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Clarence

STUDIES IN CLINICAL MEDICINE

"This continuance of Medicinal History . . . . I understand "neither to be so infinite as to extend to every common case, nor so "reserved as to admit none but wonders; for many things are new in "the manner which are not new in the kind; and if men will intend "to observe, they shall find much worthy to observe."

The Proficience and Advancement of Learning—Book II.

STUDIES IN CLINICAL MEDICINE

BY

C. O. HAWTHORNE, M.D.

Fellow of the Royal Faculty of Physicians and Surgeons of Glasgow;
Lecturer on Medicine Medical Graduates' College and Polyclinic
London; Physician to the North-West London and Royal
Waterloo Hospitals: Formerly Examiner in the
Universities of Aberdeen, Edinburgh,
and Glasgow

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PREFACE.

Many of these Essays and Lectures have already appeared in the current medical journals and I have to acknowledge the courtesy which permits their reproduction in this volume. My thanks for such courtesy are due to the Editors of the *Lancet, British Medical Journal, Practitioner, Clinical Journal, Hospital, Ophthalmoscope, Edinburgh Medical Journal,* and *Glasgow Medical Journal.* I am further indebted to the Editor of the *Practitioner* for the use of the blocks for figures 17, 18, 19, 20, 39 and 40; to the Editor of the *Clinical Journal* for figures 7, 8, 35, 36, 37, 38; and to the Editor of the *Ophthalmoscope* for figures 30 and 31. The first Essay appeared as a separate volume in 1900 and is now republished with some modifications; some of the other articles are reprinted from the *Polyclinic Journal.*

All the articles have been revised and some of them have been partly re-written. They are the outcome mainly of clinical experience and teaching and are concerned for the most part therefore with clinical problems. A certain measure of repetition which marks some of the papers is almost inevitable in a collection of occasional and detached pieces dealing with allied questions and written at different dates. The index has been arranged in some detail with a view to facilitate reference to individual points.

C. O. H.

September, 1912.

CONTENTS.

PAGE

ON RHEUMATISM IN RELATION TO RHEUMATOID ARTHRITIS AND ON THE OCCURRENCE IN EACH OF SUBCUTANEOUS FIBROUS NODULES 1

ON HEBERDEN'S NODES 41

ON CARDIAC DISEASE IN RHEUMATOID ARTHRITIS 42

ON MUSCULAR ATROPHIES IN RHEUMATOID ARTHRITIS 46

ON SUBCUTANEOUS NODULES IN CASES FREE FROM BOTH RHEUMATIC AND RHEUMATOID JOINT DISEASE 50

ON THE OCCURRENCE OF RHEUMATIC FEVER WITHOUT ARTHRITIS (*Practitioner*, September, 1899) 53

THE PREVENTION OF CARDIAC DISEASE IN RHEUMATIC CHILDREN 61

ON ARTHRITIS ACCOMPANYING OPHTHALMIA NEONATORUM (*Lancet*, May 31, 1902) 67

ON THE CLINICAL SIGNIFICANCE OF DOUBLE OPTIC NEURITIS IN CHILDREN AND YOUNG ADULTS AND ON THE CAPACITY FOR RECOVERY FROM SYMPTOMS OF TUMOUR OF THE BRAIN (*Glasgow Hospital Reports*, 1900) 71

ON THE CEREBRAL AND OCULAR COMPLICATIONS OF ANÆMIA AND ON THE PROBABLE RELATION OF THESE TO THROMBOSIS (*Lancet*, September, 19, 1908) 111

ON RETINAL EMBOLISM WITH TWO CASES IN WHICH A NORMAL STANDARD OF CENTRAL VISUAL ACUITY WAS RETAINED (*Practitioner*, June, 1907) 141

ON THE CLINICAL ASPECTS OF METASTASES TO THE CENTRAL NERVOUS SYSTEM AND OTHER PARTS IN MALIGNANT DISEASE OF THE VISCERA (*Lancet*, May 11, 1907) 151

THE SPHYGMOMANOMETER AND THE SPHYGMOGRAPH (*Lancet*, February 18, 1911) 163

ON THE OCCURRENCE OF PYREXIA IN CANCER AND OTHER DISEASES OF THE LIVER AND IN CASES OF GALL-STONE (*British Medical Journal*, March 16, 1901) 183

ON ENLARGEMENT OF THE SPLEEN IN CANCER OF THE LIVER (*Edinburgh Medical Journal*, June, 1901) 205

ON THE EYE SYMPTOMS OF LOCOMOTOR ATAXIA WITH A CLINICAL RECORD OF THIRTY CASES (*British Medical Journal*, March 3, 1900) 215

ON INEQUALITY OF THE PUPILS AND IRIDOPLEGIA (*Hospital*, March 15, 1906) 229

ON PERIPHERAL NEURITIS AND RETINAL CHANGES IN DIABETES MELLITUS (*Lancet*, September, 1899) 235

PAGE
ON SECONDARY PAROTITIS (*Glasgow Medical Journal*, vol. xliv) 241
OPAQUE NERVE FIBRES IN THE RETINA (*Ophthalmoscope*, September, 1904) 251
CICATRICIAL CONSTRICTION OF THE ABDOMINAL WALL AND OF THE LEFT THIGH ATTRIBUTED TO STRANGULATION BY THE UMBILICAL CORD (*Lancet*, August 8, 1903) 257
ON SUDDEN GREYNESS OF THE HAIR 263
THE AUTHORITY OF THE PHARMACOPŒIA 269
GALENICAL PREPARATIONS 277
ON DRUG IDIOSYNCRASIES AND OFFICIAL DOSES (*British Medical Journal*, February, 10, 1906) 279

CLINICAL LECTURES.

ON A CASE OF FUNCTIONAL HEMIPLEGIA (*Clinical Journal*, December 12, 1906) 287
ON SOME CASES OF INFECTIVE ENDOCARDITIS (*Practitioner*, March, 1911) 307
ON THE DIAGNOSIS IN A CASE OF PERNICIOUS ANÆMIA (*Hospital*, October 9, 1909) 321
ON A CASE OF CANCER OF THE LIVER (*Clinical Journal*, September, 1896) 331
ON A CASE OF PYOPNEUMOTHORAX DUE TO GANGRENE OF THE LUNG (*Clinical Journal*, September, 1899) 343
ON A CASE OF ALBUMINURIA CARDIAC HYPERTROPHY AND RETINITIS 353
ON TWO CASES OF CHRONIC RENAL DISEASE ASSOCIATED WITH SECONDARY INFLAMMATIONS OF THE SEROUS MEMBRANES (*Hospital*, July 16, 1904) 361
ON A CASE OF PERIPHERAL NEURITIS WITH ŒDEMA (*Hospital*, August 27, 1904) 369
ON THREE CASES OF TABES DORSALIS (*Lancet*, September 30, 1905) 379
ON TWO CASES OF TABES DORSALIS IN WOMEN AND ON A CASE OF JUVENILE TABES DORSALIS 391
ON SOME CASES OF SYPHILIS IN MARRIED WOMEN 403
ON SOME CASES OF SYPHILITIC PERIOSTITIS 407
ON THE OPHTHALMOSCOPIC FACTS IN CASES OF ALBUMINURIA GLYCOSURIA CONGENITAL SYPHILIS AND CHLOROSIS ... 413
ON SOME CASES OF DISSEMINATE SCLEROSIS 425
INDEX 433

ON RHEUMATISM IN RELATION TO REHUMATOID ARTHRITIS AND ON THE OCCURRENCE IN EACH OF SUBCUTANEOUS FIBROUS NODULES.

THE area or province commanded by the term Rheumatism has varied widely in its scope and contents in different periods of medical history. For the most part the movements of opinion have tended to restrict the application of the term within narrower and narrower limits, though in comparatively recent times a number of phenomena previously unsuspected of any association with it, have been included, with more or less general assent, within the contents of its claim. It is only since the seventeenth century that the significance of the word has in any degree corresponded to its modern definition. Previous to that date the disease now known as articular rheumatism was placed, together with gout, and indeed with all other affections of the joints, under the term "arthritis," any distinction which was recognized being one merely of locality and degree.[1]

In so far as the actual word Rheumatism was used by ancient writers it appears to have been applied to conditions attended with a "rheum" or mucous discharge, such as would nowadays be called a "catarrh."[1] The two words, indeed, are medically no less than etymologically related, and a well-known modern author has proposed to define a "rheumatic person" as one "who is prone to catarrhal cold in his joints."[2]

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[1] On Arthritis: "When, therefore, the humour is situated in the joints of the feet only, the complaint is called podagra; but when the cause is diffused all over the body, we commonly call it arthritis, in which the vertebræ, scapulæ, jaws, and every other joint are attacked with the disease. . . . When the disease is protracted in the joints, the humours become thick and viscid, so as to form what we call tophi or chalk-stones."—Paulus Ægineta. Adams's Translation, p. 657.

"Cælius Aurelianus (fifth century) states that various as are the situations of the disease (arthritis), its nature is the same."—Scudamore, "The Nature and Cure of Rheumatism." p. 3.

According to Scudamore,[3] and in this respect he is confirmed by Sir Wm. Church,[4] the earliest use of the word rheumatism in anything like its modern sense is to be attributed to Ballonius, who died in 1616, his works[5] being published in 1642. The same writer is also credited with establishing the distinction between gout and rheumatism, a distinction which was strongly insisted on by Sydenham[6] (1624-1689), and which, of course, at the present day is recognized medical doctrine. Similarly, and with a fair measure of general agreement, other varieties of diseases of the joints have been established and classified in groups distinct alike both from rheumatism and from gout. Thus the several joint affections which accompany pyæmia, gonorrhœa, hæmophilia, certain nervous diseases, and some of the specific fevers, have at various times been removed from the rheumatic area, and placed in new positions and amidst new relationships.[1] In these instances, however, at least for the most part, the questions in debate concern the position and limits of rheumatism as this displays itself in the form of articular disturbances more or less *acute* in character. But there is also an attack upon the area of the rheumatic claim from an entirely different quarter—an attack in which the position of rheumatism as a *chronic* disorder of the joints is seriously challenged. And the contest, apart from minor issues, involves the large question whether those manifestations of articular disturbance commonly grouped under such terms as rheumatic or rheumatoid arthritis, rheumatic gout, osteo-arthritis, etc., are to be placed within or without the rheumatic boundary. The dispute here alluded to took origin fully a hundred years ago. It has produced very acute differences of opinion, and the controversy, so far from being concluded, is found in active progress in the medical writings of the present day.

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[1] "According to ancient writers, the word rheumatism seems to have been applied to affections to which they attached the idea of a 'humoral defluxion,' especially to those characterized by mucous and pituitous discharges."—"Copland's Medical Dictionary." 1858. Vol. iii, p. 609.

"Cælius Aurelianus . . . speaks of rheumatism not as a term for diseases of the joints, but as an internal defluxion of a thin humour. Galen uses the term in the same sense."—Scudamore, Preface, p. 3.

[2] Jonathan Hutchinson, *Transactions of the International Medical Congress*, 1881, London, Vol. ii, p. 93.

[3] "A Treatise on the Nature and Cure of Rheumatism." Chas. Scudamore, M.D., F.R.S. 1827.

[4] "A System of Medicine." Sir Clifford Allbutt. First Edition. Vol. iii, p. 1.

[5] "De Rheumatismo et Pleuritide." 1642.

[6] Sydenham on Rheumatism: "This disease when separated from the fever is often called arthritis (gout). Nevertheless it differs essentially from that disease."—"The Works of Sydenham." Translated by R. G. Latham, M.D. Vol. i, p. 254.

That the disease now known as rheumatoid arthritis existed from the earliest times there is not the slightest doubt, for its record is written in clear characters in various collections of human bones of almost prehistoric date.[2] Coming to comparatively modern days a reference by Sydenham[1] shows that the facts of the disease attracted his attention, though there is nothing in his writings to suggest that he regarded the condition other than as a mere variety of rheumatism. It is to Heberden (1710-1801) and to Haygarth (1740-1827) that the responsibility of first endeavouring to advance rheumatoid arthritis to the position of an independent disease must be attached. References to the condition may be found in three of Heberden's chapters.[2] In the section dealing with "Arthritis (Gout)," he describes a "malady whatever it be named," in which "the pains are less violent than in the common gout though the swellings are greater," and in which the "remarkable circumstance is that of great and lasting feebleness"; and claims that "the continuance of the pain, the return of it, and its consequences will differ so much from ordinary gout, that it is either to be called a rheumatism or should be distinguished by some peculiar name from both these diseases." In writing of "Rheumatism," Heberden says, "The chronical differs from the acute rheumatism in being joined with little or no fever, in having a duller pain and commonly no redness, but the swellings are more permanent, and the disease is of much longer duration"; and again, "in the limb affected in the chronical sort the use has at least in many cases been wholly taken away." Then in the appendix to his book he treats specifically "Of the Chronical Rheumatism," which, he writes, "often passes under the general name of rheumatism and is sometimes supposed to be the gout," but "is in reality a very different distemper from the genuine gout and from the acute rheumatism, and ought to be carefully distinguished from them both; . . . it would be useful," he adds, " if it were distinguished by a peculiar name which might prevent it being confounded with other disorders, as by being called a spurious and wandering gout or a chronical rheumatism." Haygarth,[1] in his two papers dealing with the subject, shows an equally distinct appreciation of the clinical facts, and proposes the separation from both gout and rheumatism of a chronic affection of the joints under the term " Nodosity of the Joints." " The term rheumatism," he writes, " has been applied without sufficient discrimination to a great variety of disorders, which, except pain, have but few symptoms that connect them together." He separates rheumatism from " tic douloureux, sciatica, and lumbago, . . . which nosologists have placed under this denomination," and announces that his experience has convinced him that " there is one painful and troublesome disease of the joints of a peculiar nature, and clearly distinguished from all others by symptoms manifestly different from the gout and from acute and chronick rheumatism." It is for this condition which has " hitherto passed under the name of gout or rheumatism, or perhaps has more commonly been called rheumatick gout," that he proposes the name, *nodosity of the joints*. Among its features he describes the gradual enlargement of " the structures which form the joint," so that there are formed " nodes . . . which feel as if they were enlargements of the bones themselves "; the gradual spread of the disease " to many joints without recession in those first affected, and without shortening the patient's life "; the existence of " a crackling noise when the joints are moved "; and the slow " distortion, and even dislocation " of the joints affected. And in a paper[2] read before the College of Physicians in 1812, Haygarth emphasizes the distinction between chronic rheumatism and nodosity of the joints, pointing out, *inter alia*, that " the pain of chronic rheumatism is not accompanied with any tumour of the morbid part." For treatment, Haygarth advised repeated leeching of the joints and the use of warm baths and douches, which may "perhaps be employed with most advantage at Bath or Buxton," but as he finds that "many valuable matrons cannot conveniently leave their houses or desert their domestic duties," he suggests methods of treatment suitable for employment in these special circumstances.

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[1] There are no doubt dissentients from this general statement, at least as regards the inclusion of some of the instances named. For example, Sir Jonathan Hutchinson would not allow that gonorrhœal synovitis is non-rheumatic, but then the definition which he applies to the word rheumatism is a very wide one [" Under the term rheumatism we include all arthritic maladies which are not proved to be gouty. . . . Whenever arthritis occurs in connection with food we call it gout, and whenever such association is wholly absent we name it rheumatism "—Hutchinson, *Transactions of International Medical Congress*, 1881, Vol. ii, pp. 92, 93], and those who would enter the lists against him must either accept his definition or propose an alternative; and there are difficulties about either course. The attacks of synovitis which are sometimes associated with scarlet fever have long been a subject of controversy. The arguments and opposing conclusions are well stated on the one side in Garrod's " Treatise on Rheumatism," and on the other in " A Manual of Infectious Diseases," by Goodall and Washbourn, 1896.

[2] See article by Senator in Ziemssen's " Cyclopædia of the Practice of Medicine." Vol. xvi. p. 147. Also article by Eve, *British Medical Journal*, 1890, Vol. i, p. 423.

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[1] Sydenham on Rheumatism: "The pains . . . may cease of their own accord; the patient, however, shall be a cripple to the day of his death, and wholly lose the use of his limbs; whilst the knuckles of his fingers shall become knotty and protuberant (as in gout), with the knots showing most on the inside. With all this the stomach shall be strong, and the patient be sound in other respects." *Op. cit.* Vol. i, p. 255.

[2] "Commentaries on the History and Cure of Diseases." Wm. Heberden, M.D. London, Second Edition, 1803.

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[1] "A Clinical History of Acute Rheumatism." "A Clinical History of Nodosity of the Joints." Jno. Haygarth, M.D. 1805.

[2] *Medical Transactions of the College of Physicians*, Vol. iv, p. 294.

Towards the departure thus inaugurated by Heberden and Haygarth various attitudes have been adopted, but on the whole, opinion has steadily grown, at least in this country, in the direction of their views.

Brodie[1] was inclined to regard the disease now under consideration as gouty in nature, though he allows that there are features in its manifestations which justify the term "rheumatic gout."

On the other hand, Scudamore,[2] in his treatise on rheumatism published in 1827, when dealing with Haygarth's position, expresses the opinion that the condition termed nodosity of the joints is merely "a species of chronic rheumatism." Many of the passages in his book show very distinctly his familiarity with the more prominent features of the disease now commonly called rheumatoid arthritis. Thus, in speaking of the various structures of the joint concerned in the rheumatic process, he expresses the opinion that the cartilages are rarely attacked by the same primary inflammatory changes which affect the ligaments, and then adds, however, that "in the ultimate consequences of protracted and inveterate rheumatism the cartilage becomes involved in the disease, even to the extent of change of structure"; and that, so far as this part of the joint is concerned, "erosion and partial absorption constitute the principal kind of morbid alteration proceeding from rheumatism." (*Op. cit.*, p. 33) And again, "the fingers present the most frequent instances and the most remarkable of such distortions . . . as indicate diseased organization in all the textures which compose the joint" (*Op. cit.*, p. 34); also, "I have met with cases in which the knees have been almost wholly locked with ankylosis, and the extraordinary grating produced on attempting to move the joint seems such as might arise from a roughened state of the cartilage." (*Op. cit.*, p. 34) These passages can leave no doubt that it was in the light of a careful and exact experience that Scudamore adhered to his view in favour of the essentially rheumatic nature of the various conditions he describes.

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[1] "On Diseases of the Joints." First Edition, 1818.
[2] *Op. cit.*

In a paper on the symmetry of disease (1842), Wm. Budd[1] figures the hands of a patient manifestly distorted with osteo-arthritis, and remarks on the absorption of the articular cartilages, the alteration of the heads of the bones, the presence of ankylosis and dislocations, and adds "this affection is much less prone to shift than gout or common rheumatism; it is more common in women than in men, it seldom comes on before middle life, and it is occasionally excited by the puerperal state."

Todd,[2] after alluding in detail to the principal clinical features of the disease, which he recognizes as the condition described by Haygarth, states his preference for the term "chronic rheumatism of the joints," adding that in his experience the manifestations of the disease are "clearly traceable to a rheumatic state of the constitution."

Watson,[3] also, describes the disease as a form of chronic rheumatism, and dwells upon the symmetrical arrangement of the deformities it produces as a proof of the "constitutional origin of the disorder" and "the infection of the blood."

On the other hand, the majority of English medical writers since the middle of the nineteenth century have maintained that rheumatoid arthritis is neither a form of rheumatism nor a form of gout, but is an independent disease and entitled to a distinctive nosological position.

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[1] *Medico-Chirurgical Society's Transactions*, 1842, p. 160.

[2] "Practical Remarks on Gout, Rheumatic Fever, and Chronic Rheumatism of the Joints." R. B. Todd, M.D., F.R.S. 1843.

[3] "The Principles and Practice of Physic." Sir Thomas Watson, M.D. Fourth Edition, p. 751; First Edition, 1843.

Fuller,[1] without qualification, asserts that "Rheumatic gout is not a mere variety of gout or rheumatism, nor is it a compound of the two diseases; it is essentially distinct from them both, has a special pathology of its own, and requires a distinctive title." In the classical study of the disease by Adams[2] of Dublin, a similar though perhaps somewhat less confident view is maintained; and A. B. Garrod,[3] who first proposed the name "rheumatoid arthritis," throws the weight of his authority into the same scale.

With what general sympathy this teaching has been received is at once evident when the pages of the more recent text-books on medicine are consulted. Hilton Fagge,[4] Bristowe,[5] and Sir Wm. Osler,[6] for example, all adhere more or less decidedly to the doctrine that rheumatoid arthritis is not rheumatism; though voices on the other side are by no means silent. Thus Sir Jonathan Hutchinson's[7] view is that there exists "a state of tissue health transmittable by inheritance which involves liability to inflammations of joints and fibrous structures, and upon this arthritic diathesis as a foundation may be built up, under the influence of special causes, a tendency to gout, rheumatism, or any of their various modifications and combinations"; also that "much of what is called rheumatoid arthritis . . . is the result of inheritance of tendency to both diseases." According to Sir Dyce Duckworth,[1] though rheumatoid arthritis possesses certain distinctive features "the disease is a form of true rheumatism," and "one of several manifestations of the rheumatic branch of the basic arthritic diathesis." The term "rheumatic gout" he believes to be quite inapplicable save for a minority of cases where true coalescence occurs. Mitchell Bruce[2] is fully convinced that the "two diseases are expressions of one morbid process which differ from each other partly in intensity, and partly in the manner of their evolutions."

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[1] "Rheumatism, Rheumatic Gout, and Sciatica." H. W. Fuller, M.D. Third Edition, p. 332; First Edition, 1852.

[2] "Rheumatic Gout or Chronic Rheumatic Arthritis of all the Joints." R. Adams. First Edition, 1857.

[3] "Gout and Rheumatic Gout (Rheumatoid Arthritis)." A. B. Garrod, M.D. 1859.

[4] "The Principles and Practice of Medicine." Third Edition. Vol. ii, p. 729.

[5] "The Theory and Practice of Medicine." Seventh Edition, p. 916.

[6] "The Principles and Practice of Medicine." Second Edition, p. 305, *et seq.*

[7] *Transactions of the International Medical Congress*, London, 1881, Vol. ii, pp. 95-99. See also "The Pedigree of Disease," pp. 126-127, by the same author.

Further, there is the great authority of Charcot,[3] who, whilst he allowed that there were different types of chronic rheumatism, and that one of these might be named nodular or progressive chronic articular rheumatism, stated his opinion "that there are not two fundamentally distinct diseases to be dealt with as certain authors fancy, but only two manifestations of one and the same diathetic state." Charcot, it may be noted, denied the accuracy of the statement made by several authors that in rheumatoid arthritis cardiac lesions do not occur. He held, indeed, quite the contrary view,[4] and thus necessarily could not allow a distinction which his experience convinced him was not founded on fact.

Trousseau[5] was much less decided in his views. He was convinced that "nodular rheumatism" was neither gout nor rheumatism, though he recognized certain facts which he held "decisively connect the nodular malady with the rheumatic diathesis," and one of these was the occasional occurrence in nodular rheumatism of extensive inflammatory lesions of the pericardium.

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[1] Heath's "Dictionary of Practical Surgery." Vol. i, p. 294. See also the same author in *British Medical Journal*, 1884, Vol. ii, p. 263.

[2] Quain's "Dictionary of Medicine." Second Edition. Vol. ii, p. 662.

[3] "Lectures on Senile Diseases." Sydenham Society's Translation, pp. 154, 163, 180.

[4] *Op. cit.*, p. 173 *et seq.* See also p. 42 of present volume.

[5] "Lectures on Clinical Medicine. On Nodular Rheumatism, erroneously called Rheumatic Gout." New Sydenham Society's Translation. Vol. iv, p. 410, *et seq.*

But the position of those who deny the rheumatic nature of rheumatoid arthritis has not been confined to mere denial. On the contrary, there has been advanced at least one very confident explanation of its etiology and nature which has received a considerable measure of support. It is hardly within the scope of this essay to deal in detail with "explanations" of rheumatoid arthritis except in so far as these involve a negation of the identity of the disease with rheumatism; and therefore only a brief allusion is here made to this part of the subject. It is sufficient to refer to the fact that the symmetrical manifestations of the disease, the existence of muscular atrophies and other suggestions of trophic disturbance, the recognized analogies between the joint conditions and articular disturbances known to depend on spinal lesions, the at times exaggerated tendon-jerks, and the apparent sequence of the disease on circumstances which have an exhausting influence on nerve force, have all been advanced in support of an interpretation which would regard the whole of the phenomena as the results of disturbed innervation. This view of the disease was first offered by Remak (1858), who proposed the name "arthritis myelitica," though it is not uninteresting to note that long before this date Scudamore (*Op. cit.*, p. 339) had suggested that the conditions were due to "a slow perversion of nutrition."

The dystrophic hypothesis has received the adhesion and support of Senator,[1] Ord,[2] Garrod,[3] Osler,[4] Hyde,[5] Kent-Spender,[6] and others. Opinions differ as to the exact part of the nervous system in which the primary disturbance originates, but the more frequent occurrence of the disease in women, and its admitted development at periods of life when menstrual disturbances and other evidences of intra-pelvic disorders are not uncommon, have suggested the view that the disease, at least in a number of cases, may be of the nature of a reflex tropho-neurosis. Ord[1] argued strongly in favour of this suggestion, though he believed that the lesion in some cases was a primary one in the spinal cord itself; and the facts as to the sex relations of the disease have attracted general attention quite apart from any theory. Haygarth, Todd, Fuller, Charcot, Trousseau, and others, draw attention to the greater frequency of the disease in women and to its associations with abnormal uterine conditions, though on this aspect of the subject A. E. Garrod remarks that he has found men to be frequent sufferers, and he therefore doubts whether any special stress can fairly be laid on uterine disturbances as a cause of the disease.

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[1] "Ziemssen's Cyclopædia of the Practice of Medicine." Vol. xvi, p. 152.

[2] *British Medical Journal*, 1880, Vol. i, p. 155.

[3] "A Treatise on Rheumatism." A. E. Garrod, M.D. 1890.

[4] "The Principles and Practice of Medicine." Second Edition, p. 305.

[5] "The Causes and Treatment of Rheumatoid Arthritis." S. Hyde, M.D. 1896.

[6] "Osteo-Arthritis." J. Kent-Spender, M.D. 1889. See also same author in Sir Clifford Allbutt's "System of Medicine." First Edition. Vol. iii, p. 173.

Another and more recent doctrine of the nature and causation of rheumatoid arthritis is the one which teaches it to be dependent on a specific micro-organism[2]; and yet again, it is claimed that whatever be the exact nature of the disturbing influence, the essential fault is in "the internal secretion of the structures of the joint,"[3] this view carrying with it the almost inevitable suggestion that patients may be usefully treated by the administration of "an extract from the articular cartilages and synovial membranes of healthy animals."

No doubt these more positive positions have in some sense strengthened the standing of those who contend against the rheumatic nature of rheumatoid or osteo-arthritis, if only for the general reason that an affirmation usually commands more support and sympathy than an attitude of mere negation, though it is not suggested here that the advocates of these views have not some more valid basis than this for their defence. The division of opinion, however, remains, and the resolute attitude of those to whom rheumatoid arthritis is simply a form of rheumatism has not been modified because their opponents have been able to set up a competing positive hypothesis. Still it must be admitted that the dystrophic doctrine of rheumatoid arthritis has made a not unsuccessful appeal to many of the leaders of professional opinion, and that it is one of a series of movements by which the extent of the authority of the term rheumatism over abnormal conditions of the joints has, in the judgment of a large constituency, been seriously diminished. There are, however, compensations; for, though, as a disease of the joints, the boundary of rheumatism may have been pushed back, the enlarged claims advanced at different dates during the last century in reference to the area of the disease in other directions, have carried its authority into new and important territories. And some of these claims are hardly in dispute.

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[1] *Transactions of the Clinical Society*, Vol. xii, 1879; also *Lancet*, 1887, Vol. ii, p. 1067.

[2] "Rheumatoid Arthritis: Its Pathology and Treatment." Gilbert Bannatyne, M.D. Second Edition.

[3] "The Causes and Treatment of Rheumatoid Arthritis." S. Hyde, M.D. p. 78.

Certainly it will not be denied that endocarditis and pericarditis are frequently, and indeed usually, as true manifestations of the rheumatic influence as synovitis. Chorea, tonsillitis, erythema multiforme, and erythema nodosum, are in a position scarcely less secure, and authorities whose views must be received with every respect would include pleurisy, peripheral neuritis, iritis, purpura, meningitis, pneumonia, and indeed many other conditions, as at least possible members of the rheumatic series.[1] There remains to be added to this list the occurrence of subcutaneous fibrous nodules. And the addition is the more important to the present purpose because the claim has been made that whilst the majority, if not all, of the conditions enumerated above may result from influences other than those which produce rheumatism, the eruption of fibrous tumours in the subcutaneous tissue is an event which in any given case closes the discussion. It is a final and unequivocal proclamation in favour of rheumatism. Moreover, the fibrous nodule has been held to influence prognosis as well as to determine diagnosis, and has even been dignified as the typical rheumatic structure. Thus it has been detected in characteristic form in the thickening of rheumatic pericarditis;[1] has been regarded as the homologue of the endocardial vegetation;[2] and has been offered as justification for a suggestion that the manifestations of chorea depend upon proliferative changes in the neuroglia.[3] Because of this position, and bearing in mind the antagonistic views held in reference to the rheumatic or non-rheumatic nature of rheumatoid arthritis, it becomes a matter of great interest to inquire whether, in any undoubted cases of this disease, subcutaneous fibrous tumours do or do not occur. For, if they do so occur, one of two conclusions is inevitable. Either rheumatoid arthritis is rheumatism, or the development of fibrous tumours in the subcutaneous tissue is not a special and distinctive note of rheumatism. Once grant the appearance of such tumours in admitted cases of rheumatoid arthritis, and from the dilemma just presented there is no escape.[4] It is not the ambition of this essay to suggest which alternative should be selected, but rather to collect and to exhibit evidence to show that in cases which are beyond dispute cases of rheumatoid arthritis there may and do appear fibrous tumours which are essentially the same as those affirmed to be characteristic and decisive results of true and undoubted rheumatic disease.

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[1] *See* Bibliography in Garrod's "Treatise on Rheumatism"; "The Rheumatic State in Childhood," W. B. Cheadle; "On Peripheral Neuritis," Jas. Ross and Judson Bury; and Barlow on "The Forms of Rheumatism," *British Medical Journal*, 1883, Vol. ii, p. 511.

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[1] Cheadle. *Op. cit.*, p. 89. *Pathological Society's Transactions*, Vol. xxxiv, p. 45. Angel Money, *Lancet*, Vol. ii, p. 265, 1893.

[2] *Lancet*, *Ibid.*, p. 118 (Dawtrey Drewitt.

[3] Cheadle. *Op. cit.*, p. 67.

[4] At least, there is no escape so long as all cases of osteo-arthritis are regarded as included in one and the same etiological category. If, however, it is allowed that the condition of osteo-arthritis is merely symptomatic, and that it may result from several independent causes, the claim may be made that some cases are and some are not rheumatic. But, as the sequel will show, the occurrence of subcutaneous fibrous tumours is by no means limited to those cases of rheumatoid arthritis which carry in their history suggestions of rheumatism in its generally admitted forms. Hence, for these cases, at all events, the dilemma as above stated clearly remains.

The existence of swellings or tumours in the immediate neighbourhood of joints affected by what is now called rheumatoid arthritis, was noted in some of the earliest descriptions of the disease, and even in descriptions written prior to any attempt to separate the condition from rheumatism. Sydenham, as already quoted, speaks of the knuckles becoming "knotty and protuberant (as in gout)." Heberden's chronical rheumatism is distinguished from the acute variety by its "more permanent swellings"; whilst to Haygarth, the special feature of the disease was the development of the "nodes" which led him to suggest the term "nodosity of the joints." That these "nodes" were osseous in character there can be little doubt. Haygarth himself says of them "they feel as if they were enlargements of the bones themselves" (*Op. cit.*, p. 155); and as he found them in connection with all the joints (*Op. cit.*, p. 153), it is reasonable to conclude that his descriptions apply to those bony outgrowths which in more recent times have been termed "osteophytes."

The swellings which are found, not infrequently, at the bases of the terminal phalanges of the fingers, and which are known as "Heberden's nodes," may here be noted, because, though Heberden himself did not suggest that they had any relation to his "chronical rheumatism," they have been claimed as one form of the disease now under consideration. Heberden[1] described them under the term "digitorum nodi," and confined his assertions concerning them to the statement that "they have no connection with gout, being found in persons who never had it," but Charcot, A. B. Garrod, Kent-Spender, Osler, and A. E. Garrod, all agree that these nodes are a variety of the disease which the first-mentioned considered to be merely chronic rheumatism, and the others regard as a special disease, namely, osteo- or rheumatoid arthritis.

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[1] See Note, p. 41.

The elaborate study of the clinical history and pathological anatomy of "rheumatic gout" conducted by Adams[1] of Dublin demands attention and regard from many points of view, and among these most certainly is the question of tumours or swellings in the neighbourhood of the affected joints. In dealing with this point Adams sets out with the statement that it is common to find bursal enlargements near the joints. Some of these, he says, are the normal bursæ morbidly developed into fluctuating tumours; others are, "as it were, accidentally developed in the cellular tissue." (*Op. cit.*, p. 17) In these bursæ are often to be found some of the "foreign bodies" which, as Adams insisted, are so frequently present, and in great numbers, within the joint cavities, and which he regarded as developed from the synovial membranes. But in one of his cases, described in detail (*Op. cit.*, p. 38), some of the "foreign bodies" were present not only within the knee-joint but also "outside the synovial membrane in the popliteal region." Adams also recognized yet another way in which firm fibrous masses may develop outside the joint, for he states that as the disease advances the fluid contained in the extra-articular bursæ may be absorbed, and the bursæ be converted into small solid tumours.

He describes one case in which a tumour "quite superficial, about the size of a hen's egg," was present on the inner aspect of the upper extremity of the tibia; this, when cut into, was "found to be intersected by delicate membranous bands resembling chordæ tendineæ of the heart" (*Op. cit.*, p. 398); and again, after describing the distention of the bursa which normally exists at the decussation of the semimembranosus with the internal head of the gastrocnemius, he adds that

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[1] "Rheumatic Gout or Chronic Rheumatic Arthritis of all the Joints." Third Edition, 1873.

"in the ordinary course of the disease the redundant quantity of the fluid is absorbed, and the popliteal tumour disappears"; and he quotes a case in which the obliterated popliteal bursa had "its walls converted into a small solid tumour about the size and shape of an almond." (*Op. cit.,* p. 197) This last case is figured in his Atlas,[1] where also are to be found other drawings representing tumours associated with joints affected with "rheumatic gout." One of these, it may be noted, is said to be "ossific," and another, which is shown as a subglobular projection from the dorsal aspect of the olecranon, is described as "a large additamentary bone placed in apposition with the olecranon posteriorly." (Atlas, plate iv, fig. 5)

There are thus, according to Adams, three forms of solid tumours which may be met with near to joints attacked by rheumatic gout, namely: (1) additamentary bones; (2) foreign bodies outside the synovial membrane; and (3) fibrous nodules, which he regards as formed by absorption of fluid from, and thickening of the walls of, extra-articular bursæ. It is of interest in this connection to observe that Scudamore[2] has a special chapter on "Chronic Rheumatism of the Bursæ," in which he says that these structures may become indurated in a remarkable degree, so indeed as to acquire sometimes the condition of bony hardness, and that under such circumstances "the complaint may take the name of nodosity." Writing of chronic rheumatism generally, the same author says "we sometimes find the bursæ enlarged, thickened and hardened." (*Op. cit.*, p. 348) Fuller[3] merely remarks that the disease, rheumatic gout, usually causes thickening and permanent enlargement of the parts, but does not allude to any distinct nodules or tumours. Charcot's selection of the term "nodular rheumatism" would appear to be based on osteophytic or other enlargements of the ends of the bones, for he speaks of "bony nodes which de-form the ends of the bones" (*Op. cit.*, p. 181); and of nodosities that "pierce the underlying skin . . . as in gout." (*Op. cit.*, p. 189) Charcot alludes also to "foreign bodies" in the joints and to osteophytes, but does not describe any fibrous or bursal tumours near the joints. According to Senator[1] "hard excrescences and nodular masses may not infrequently be detected round the joints," and in some cases "more or less painful thickenings of variable size" are found "in the soft parts of the upper arm or forearm"; these thickenings, he suggests, may possibly be due to a circumscribed proliferation of the connective tissue between the muscles or to swellings in the course of the nerves. Fagge[2] mentions as one of the less common symptoms of osteo-arthritis the presence of "fibrous nodules" at a distance from the joints, as for example, among the muscles of the arms or forearms; and Lyman[3] describes "fibrinous nodules" in similar situations, but neither of these authors speaks of the nodules as "subcutaneous," and Lyman insists upon their distinction from the "nodule of articular rheumatism." Several other text-books of medicine may likewise be consulted without finding any reference to the occurrence of subcutaneous fibrous tumours in rheumatoid arthritis. Even some of the more special monographs do not allude to their existence. Thus Hyde, in the work already quoted, whilst he describes "increased secretion in synovial capsules or adjacent bursæ, and swellings of cartilages, ligaments, tendons, and the fibrous attachments of muscles" (*Op. cit.*, p. 14), makes no mention of fibrous tumours. The same is true of Lane and Griffiths,[4] who, in 1890, published an analysis of 1,200 cases of chronic joint affections with a view to establish a distinction between rheumatic and rheumatoid disease.

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[1] "Illustrations of the Effects of Rheumatic Gout." Second Edition, 1873.

[2] *Op. cit.*, p. 487.

[3] *Op. cit.*, p. 333.

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[1] "Ziemssen's Cyclopædia of the Practice of Medicine." Vol. xvi, p. 147.

[2] *Op. cit.*, Vol. ii, Footnote, p. 227.

[3] "A Text-book of the Theory and Practice of Medicine" 1894. Edited by Wm. Pepper, M.D. Vol. ii, p. 131.

[4] "The Rheumatic Diseases (so-called)." H. Lane and C. T. Griffiths. 1890.

There can be no doubt, however—and the statement will be substantiated in the sequel—that fibrous tumours do develop in the subcutaneous tissue in some cases of rheumatoid arthritis, and while certain broad clinical distinctions can be drawn between such tumours and the subcutaneous nodules not infrequently observed in the acute and subacute rheumatic attacks of childhood, there are intermediate forms which link together the two extremes. Such differences as exist are differences of degree and not of kind, and it therefore becomes impossible, even allowing that clinical confusion is very unlikely, to insist upon the appearance of such growths as a conclusive proof of rheumatism and at the same time to maintain the doctrine that rheumatoid arthritis is not rheumatism. Manifestly one or other conclusion must go by the board.

The special clinical features of "rheumatic nodules," as emphasized in the early history of their career, were, in addition to their undoubted rheumatic significance (1) their small size; (2) their non-occurrence after puberty; (3) their rapid evolution and temporary duration; and (4) their painlessness. On the other hand, in the majority of cases in which subcutaneous fibrous growths have been noted in rheumatoid arthritis, these have been of some considerable size, have occurred almost necessarily in patients at or near to the middle period of life, have been slow in development and of prolonged or permanent duration, and have caused more or less pain. In reference to the latter point it may be suggested that the occurrence or non-occurrence of pain depends mainly on the situation and size of the tumour, for, obviously, the more considerable the projection the more likely is the tumour to receive irritation from external influences. Further, it is certain that the larger tumours of rheumatoid arthritis are by no means invariably painful (pp. 30, 34). And in any view of the matter, the existence or non-existence of pain can scarcely be insisted upon as a vital distinction between growths which are identical in structure (p. 31), and are closely related in other respects. It may be noted, too, that in a few cases typical rheumatic nodules have been painful. One such case is reported by Judson Bury,[1] another by Futcher[2]; Mahomed,[3] also, describes a case of subacute rheumatism with large subcutaneous nodules showing evidences of more or less irritation. With reference to the other distinctions that have been drawn between the subcutaneous tumours of rheumatism and those of rheumatoid arthritis some clinical experiences are now quoted to show that these distinctions are not absolute.

(1) *Age of Patient.*—Cavafy[4] has published a case of typical nodules with rheumatic fever in a man aged 18; West,[5] one of numerous nodules with acute rheumatism in a woman aged 39; Middleton,[6] one in which the development of the nodules followed a second attack of rheumatism at 36 years; Coutts[7] has seen a woman aged 40 with rheumatism, heart disease, and numerous nodules; Futcher, in the paper already quoted, records two cases in which the nodules appeared at 41 and 47 years respectively; and Cheadle,[8] also, has met with two cases of rheumatism with subcutaneous nodules in adults.[9] On this point, too, Osler,[10] after remarking on the infrequent occurrence of rheumatic nodules in Baltimore and Philadelphia as compared with London, remarks "since 1881 I have been in the habit of looking for them . . . and I have seen a larger number of instances in adults than in children."

(2) *Size of Tumours.*—In Middleton's case, though some of the tumours were small, others were of con-

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[1] "On Peripheral Neuritis." Jas. Ross and Judson S. Bury. p. 341.

[2] *Johns Hopkins Hospital Bulletin*, October 10, 1895.

[3] *British Medical Journal*, 1883, Vol. i, p. 622.

[4] *Transactions of the Pathological Society*, 1883, p. 41.

[5] *St. Bartholomew's Hospital Reports*, Vol. xxii, p. 213, 1886.

[6] *The American Journal of the Medical Sciences*, Vol. xciv, p. 443, 1887.

[7] *Illustrated Medical News*, 1889, Vol. iii, p. 267.

[8] Cheadle. *Op. cit.*, p. 69.

[9] Some more recent illustrations are to be found in the *Transactions of the Clinical Section of the Royal Society of Medicine*, Vol. i, part 1.

[10] "On Chorea and Choreiform Movements." Sir Wm. Osler, M.D. 1894.

siderable size; Cheadle has, in extreme instances, found rheumatic nodules of the size of half a walnut (*Op. cit.*, p. 69); in one of Futcher's cases the nodules varied from the size of a pea to that of a walnut; and Coutts describes a case in which the rheumatic nodules were "open, gross, and palpable excrescences." Dr. D. B. Lees (*Transactions of the Society for the Study of Disease in Children,* Vol. ii, p. 5) notes "very large rheumatic nodules" in a boy aged 9, the nodules disappearing under the administration of sodium salicylate. On the other hand, in three of the cases (pp. 30, 34, 39) submitted with this essay—cases, it is claimed, which correspond exactly to authoritative descriptions of rheumatoid arthritis—there existed some tumours certainly not exceeding in size the nodules described as typical of acute rheumatism. Newton Pitt,[1] too, has published a case of ostœ-arthritis with both large and small tumours, the latter being the size of small shot. In reference to the size of the subcutaneous nodules of rheumatism, Futcher, in the paper already quoted, comes to the conclusion that there are two varieties, which differ from one another in structure as well as in size. The first is comparatively small, extremely firm, distinctly rounded, and easily movable beneath the skin. This is the typical rheumatic nodule, and is composed of fibrous tissue. The second and less frequent variety attains a larger size, is softer in consistence, somewhat flattened and lobulated, and is occasionally slightly adherent to the skin. This variety Futcher considers to be fibro-lipomatous in structure. In Case 5 recorded (p. 38) with this essay it will be found that the numerous small subcutaneous tumours present have exactly the characters just quoted. But that case is not one of acute or subacute rheumatism. It adheres rather to the type of rheumatoid arthritis. This is seen in the nodular osseous masses at the bases of certain of the metacarpal bones, in the presence of Heberden's nodes, in the chronicity of the condition, and in the development of the symptoms about the time of the menopause and after a habit of frequent child-bearing extending over a number of years. The case is therefore of considerable interest as showing that a variety of subcutaneous tumours, described by a previous observer as occasionally occurring in rheumatic patients, may also be found in association with symptoms which suggest osteo-arthritis rather than unequivocal rheumatism. And such an association is obviously a matter of much significance in connection with the question now under review.

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[1] *Transactions of the Clinical Society*, Vol. xxvii, p. 54.

(3) *Duration of Tumours.*—It is of course to be expected that in a chronic affection like rheumatoid arthritis the process of tumour formation, if it occurs, should partake of the general obstinacy of the disease, and should contrast in this respect with the more fleeting phenomena of an acute or subacute rheumatic development. Yet even here an absolute distinction between the subcutaneous growths which at times mark the two conditions cannot be erected, for, in the cases described with this essay there is evidence of the diminution in size, and even of the complete disappearance, of tumours in rheumatoid arthritis; Garrod,[1] also, reports a case of osteo-arthritis (the patient having had a slight attack of rheumatism fifteen years previously) in which tumours present in the subcutaneous tissue diminished in size under observation; Payne[2] has seen subcutaneous tumours completely disappear in a case of rheumatoid arthritis; and in one of Newton Pitt's cases the disappearance of some of the tumours was associated with the development of others in new situations. As opposed to this, the nodule of acute rheumatism has been shown at times to be much more persistent than was at first believed to be the case. The maximum duration in Barlow and Warner's cases was five months, and this was regarded as quite exceptional. But in one of Cavafy's cases the nodules persisted for at least a year; and in Middleton's case they were still present three years after the attack of rheumatic fever. Among Futcher's cases is one of a man aged 49 in whom

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[1] "Twentieth Century Practice of Medicine." Vol. ii, p. 452.
[2] *Lancet*, 1883, Vol. i, p. 545.

rheumatic nodules had been present for eight years, a second in which they had had a similar duration, the patient being 55 years, and a third, a man aged 28, in whom some of the nodules had persisted for no less than fifteen years. Coutts, also, has recorded his impression that in adults, rheumatic nodules, when they occur, are much more persistent than they are in younger patients.[1]

There is, therefore, a considerable body of evidence in favour of the contention that neither as regards the period of life at which they occur, nor in respect to their size, nor in the characters which mark their duration, can any rigid line of distinction be drawn to separate the so-called rheumatic nodules from the subcutaneous tumours which have been noted in occasional cases of rheumatoid arthritis.

In the foregoing paragraphs a general reference has been made to the evidence of recorded cases of subcutaneous tumour formation in rheumatoid arthritis. These records may now be stated in a more systematic fashion. In 1883, Sir Dyce Duckworth[2] showed to the Clinical Society a woman, aged 24, with subcutaneous nodules on the hands and knees, the patient being the subject of "chronic, painful swellings of the joints"; cracklings were felt on moving the knee- and shoulder-joints, and Heberden's nodes were present on the fingers. There was neither personal nor family history of rheumatism, but the first cardiac sound was "roughened and reduplicated." In the same year Payne,[3] in a discussion at the Pathological Society, described the existence of nodules in the skin in cases of rheumatoid arthritis, and Hutchinson and Mahomed each quoted individual experiences in support of this statement. Howard,[4] in "Pepper's System of Medicine," published in 1885, says, in so many words, that "the so-called rheumatic nodules occur also in rheumatoid arthritis." Middleton's case was recorded in 1887, and although the patient had had two attacks of rheumatic fever, the fact that crepitation was present in the joints of the fingers, and that there were movable bodies in the wrist-joints, would, according to the majority of authorities, justify the view that the case had become one of rheumatoid arthritis; for, even those who are most opposed to the rheumatic nature of this disease allow that not infrequently it develops after rheumatic fever. Leonard Weber,[1] in 1888, reported the case of a woman, aged 37, who was the subject of osteo-arthritis, and in whom subcutaneous nodules were present in the upper limbs. One of the most striking cases in relation to the matter now under consideration was recorded by Wallace Anderson[2] in 1891: the patient, a woman aged 50, displayed the characteristic deformities of rheumatoid arthritis, whilst over the backs of the hands and toes were small nodules, and much larger masses over each olecranon; there was no cardiac disease, and no personal or family rheumatic history.[3] In 1893 Hector Mackenzie[4] described the case of a man, aged 43, who suffered from a disease undistinguishable from acute rheumatoid arthritis, with nodules all over the scalp and body; post mortem examination showed fibrous nodules on the mitral valve and pericardium and in the substance of the kidney. The patient had had four attacks of acute rheumatism in early life. Newton Pitt,[5] in 1894, published three cases of osteo-arthritis with subcutaneous nodules; in one of these there had been "acute arthritis" some years previously, but the others were free from suspicion of ordinary rheumatic manifestations. In addition to the nodules, one of the patients had diffuse thickening of the subcutaneous tissue of the hands with a degree of Raynaud's phenomenon and sclerodermia; Middleton's case, also, showed widespread induration of the soft tissues of the hands. A. B. Garrod,[1] in 1895, put on record the case of a man, aged 39, with osteo-arthritis, subcutaneous tumours, and freckles on the hands, the last-named symptom being, according to Kent-Spender, a most suggestive circumstance towards the diagnosis of rheumatoid arthritis. In this case, however, the patient was by no means free from suspicion in reference to "ordinary rheumatism," for he had had a slight attack of rheumatism when aged 24, and there were evidences of cardiac valvular disease. Such an association in any particular case must necessarily lessen to some degree the value of the case as a proof that subcutaneous tumours occur in rheumatoid arthritis, because it leaves a manifest possibility that the tumours were the result of an ordinary rheumatic attack. But in some of the cases quoted above there is no room for this suggestion, and the cases recorded with this essay are singularly pure in this respect. Attention from this point of view may be particularly paid to Case I (p. 27), for, it is not too much to say that if there ever existed a case of rheumatoid arthritis which was not rheumatism, this is the case. The man's personal and family records are practically free from rheumatic incidents; until he is 40 years of age he enjoys good, and even robust health; his illness, which begins with pains in the feet, follows a period of considerable mental anxiety and strain; the manifestations of articular disturbance show marked symmetry, and, beginning in the peripheral joints of the limbs, gradually approach the trunk; the deformities of the hands exactly correspond to the classical descriptions of rheumatoid arthritis; there is evidence during the course of the case of implication of the temporo-maxillary articulation; some of the joints give coarse grating on movement; though muscular atrophy is not extreme, it exists; some of the tendon-jerks appear to be exaggerated; the pulse is unduly rapid (no cardiac disease); and there are local sweatings of the hands and feet. Yet with this combination of features, every one of which has been urged more or less strongly as giving a special character to rheumatoid arthritis, there develop subcutaneous tumours, some of which are no doubt larger than the usual "rheumatic nodule," but others, even as regards size, exactly correspond to that structure. Further, there are also nodular thickenings in some of the tendons, and according to the patient's judgment at least, some of the tumours in the skin had but a temporary existence. Moreover, it happens that in this case we have an actual knowledge of the structure of the subcutaneous tumours (fig. 3), and this is seen to be identical with the structure of the "rheumatic nodule" as described and figured by Cheadle, Cavafy, and Middleton. It is impossible, therefore, to imagine a more complete demonstration of the accuracy of the proposition that subcutaneous fibrous tumours may be found with rheumatoid arthritis even when the record and the condition of the patient are otherwise entirely free from evidence of rheumatic tendencies or manifestations. Detailed reference to the other cases here reported is not necessary. They support more or less definitely the testimony which is offered by the case just described, and help to form a distinctive body of evidence in favour of the conclusions that subcutaneous fibrous tumours develop in rheumatoid arthritis as well as in acute and subacute rheumatism; that whilst certain broad distinctions hold good between the nodules which are found in rheumatism and rheumatoid arthritis respectively, these distinctions are not absolute; and that it is necessary to bear in mind the fact of such tumour formation in discussing the mutual relations of the two conditions.

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[1] Other illustrations of the accuracy of this view may be found on reference to the *Transactions of the Royal Society of Medicine* (Clinical Section), Vol. i, part 1.

[2] *Transactions of the Clinical Society*, Vol. xvi, p. 52.

[3] *British Medical Journal*, 1883, Vol. i, p. 622.

[4] "Pepper's System of Practice of Medicine." Vol. ii, p. 84.

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[1] "Reference Handbook of the Medical Sciences." Vol. i, p. 367.

[2] *Glasgow Medical Journal*, March, 1891.

[3] A somewhat similar case was shown to the Medical Society of London by Dr. F. Parkes Weber in 1906, and is figured in Vol. xxx of the Society's *Transactions*.

[4] *British Medical Journal*, 1893, Vol. ii, p. 1324.

[5] *Transactions of the Clinical Society*, Vol. xxvii, p. 54.

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[1] "Twentieth Century Practice of Medicine." Vol. ii, p. 452.

On a review of the whole question, it must then be allowed that whilst subcutaneous nodules having the characters described by Barlow and Warner are frequently and in practice of the highest clinical value as evidence of rheumatism, too sharp a conclusion has been drawn in reference to the size and duration of such nodules, and also in reference to the age at which they occur. Further, it must be recognized that in a distinct proportion of cases of rheumatoid arthritis nonsequential to acute or subacute articular rheumatism, there appear subcutaneous tumours, which, neither in their histological characters nor in other respects, can be definitely separated from the so-called "typical rheumatic nodule." It is, therefore, impossible to admit as established medical doctrine the proposition that the presence of such tumours is an infallible proof of rheumatism, without at the same time allowing that rheumatism and rheumatoid arthritis are not two distinct and different diseases, but are merely different forms or manifestations of one and the same pathological process.

CLINICAL RECORDS OF RHEUMATOID ARTHRITIS WITH SUBCUTANEOUS FIBROUS TUMOURS.

CASE I.—Robert D., aged 49 (May, 1893), complains of stiffness in his joints. He appears to have followed the development of his present condition with great care, and is evidently a very observant and intelligent man. According to his own story he enjoyed very good health until nine years ago. At that time, in consequence of a break-up of the firm for which he had worked for many years as a "power-loom tenter," he had to take a new situation, where he found his duties very heavy and trying, mainly in consequence of certain unexpected responsibilities for which his previous experience had not fitted him. He was very much worried, and feared he would have to give up the situation; his appetite failed, he could not sleep, and, as he puts it, his "body got down altogether." After this experience had continued for some months he began to complain of pain and soreness across the "tread of each foot." A few months later the left knee became stiff and painful, and similar conditions with more or less swelling gradually appeared in some of the finger-joints, the right knee, and the wrists and elbows. Much more recently he has had to complain of difficulty and pain in the movements of his shoulders and hips. He has also had pain and stiffness in the left side of his neck, as well as pain and a sense of "cracking" when moving his lower jaw.

It is to be noted that at no time has there been any acute pain. His complaint is of "a dull pain and stiffness in the joints." He dwells particularly on this last feature and on the difficulty which attends his efforts to move his limbs when first waking in the morning, the

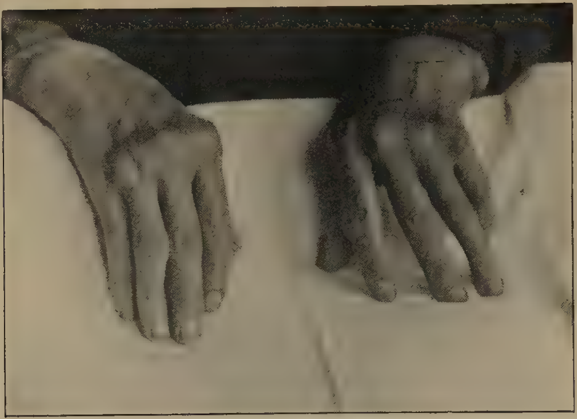

FIG. 1.—Rheumatoid arthritis, showing ulnar deflection of digits, enlargement of heads of metacarpal bones, and in the left metacarpophalangeal region a small subcutaneous tumour.

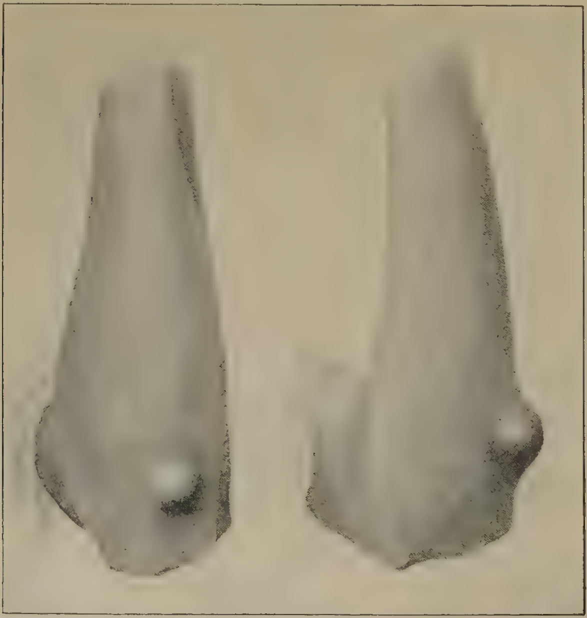

FIG. 2.—Rheumatoid arthritis, showing large subcutaneous tumours in the neighbourhood of each elbow. (From a drawing by Dr. Alex. Macphail.)

stiffness always being very noticeable after the limbs have been kept at rest for a time.

Some four years ago he had to give up his occupation, partly on account of the stiffness of his joints, but mainly, he says, because of the great decline of his general strength.

Inquiries into his family history elicit no record of rheumatism or joint troubles except the statement that he thinks that one of his grandmothers had "rheumatic joints." He is a married man, father of eight children, all living and healthy; he has never indulged in alcoholic excess; no history or evidence of venereal disease. His own health, previous to his present illness, was, he considers, unusually good.

In reference to the general condition of the patient, it may be said at once that in repeated examinations made at intervals during several years no evidence of disease in the thoracic or abdominal viscera was ever discovered. The only features of importance noted, apart from the condition of the articulations and the tumours subsequently to be described, were the tendency of the pulse to range above the average (the usual record was between 90 and 100); the frequent occurrence of attacks of local sweating, more especially on the hands; the somewhat glossy condition of the skin over the finger-tips; the existence of longitudinal ridges on the nails; the presence of two patches of psoriasis, one in front of each shin; and the existence on the skin of the forearms of several small darkly-pigmented spots, which, patient was sure, had appeared in quite recent years.

The condition of the joints need not be given in detail. The state of the hands is fairly well seen in the accompanying photograph (fig. 1). It is sufficient to add that all the joints of the limbs betrayed evidence of disease in a greater or less measure, and the "lipping" of the articular ends of some of the bones, the coarse grating on movement, the restriction of the range of movement by "bony formations," and the resulting deformities, combined to form a condition which none would have difficulty in recognizing as one of rheumatoid arthritis. The muscular condition generally was poor, but evidences of more distinct atrophy were appreciable in the thenar and hypothenar eminences, in the dorsal interossei, more especially each abductor indicis, and in the extensors over the back of each forearm. The knee-jerks were very decided and were regarded as exaggerated; in each upper limb the biceps- and triceps-jerks were present, but not to an excessive degree.

In all the above features the present case is manifestly nothing more than an ordinary example of poly-articular rheumatoid arthritis.

Attention must now, however, be given to the subcutaneous tumours, which are judged to be the special feature of the case. The largest of these are in the neighbourhood of the elbow-joints, and are well displayed in fig. 2. In addition to the considerable mass seen over the dorsal aspect of each olecranon, there is a smaller tumour over the internal condyle of the left humerus, and one also over the internal surface of the right olecranon. Each tumour is firm, is free from adhesion to the skin, and is more or less movable in the subcutaneous tissue. Except in the case of the one over the left internal condyle the skin over the tumours is not reddened. The last-mentioned tumour sometimes causes the patient pain; he attributes its irritated condition to pressure, and as the internal condyle is considerably enlarged the projection of the tumour is rendered very prominent. The history of these masses is that they have appeared at various dates during the last four years, and that they commenced as small, firm nodules, and slowly increased to their present size.

Several small firm nodules are present in the subcutaneous tissue over the dorsum of the left hand. The most prominent of these is on the dorsal aspect of the fifth metacarpo-phalangeal joint (fig. 1). They are all free from attachment to the skin, and have never caused the patient any pain or discomfort.

In the lower limbs there is a nodular mass about the size of a horse-bean attached to the posterior aspect of each tendo Achillis; these, patient says, have been present for about six months. Formerly he was aware of several other nodules along the outer side of each foot, but these have now entirely disappeared.

Patient permitted the removal of the tumour over the left olecranon. Its situation was in the subcutaneous tissue and it was not adherent to the periosteum. It is an almond-shaped mass, measuring 30 by 20 mm. and weighing 3 grms. It is of hard, resistant structure, and is found to consist of dense fibrous tissue (fig. 3).

The further history of the case may be briefly stated. Patient was seen at intervals during the next four years. The disease made some, but no very marked advance. In the scar left by the removal of the tumour a new fibrous mass soon began to appear, and gradually enlarged, but no new tumours appeared, and the existing masses remained without change. Patient died in the spring of 1897 of acute pneumonia. A post mortem examination was not permitted.

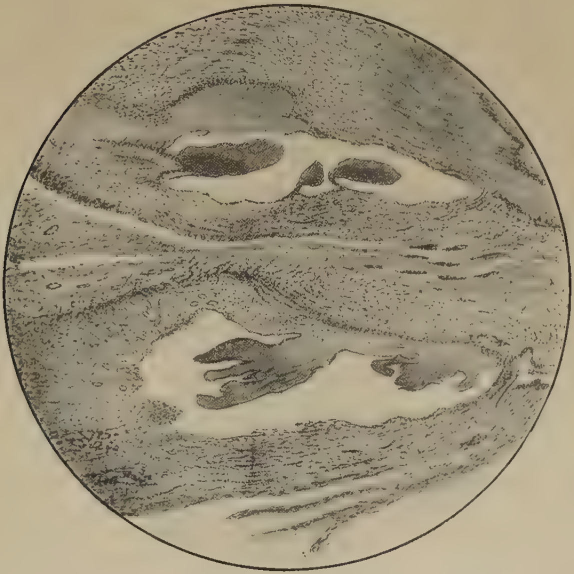

FIG. 3.—Section of subcutaneous fibrous tumour showing dense fibrous tissue. The section includes two veins with thickened walls and partly occupied by incompletely organized fibrin, presumably the result of a phlebitis accompanied by thrombosis. (See Case 1.)

There cannot be the slightest doubt that this was a case of rheumatoid arthritis. Apart from the deformities of the hands and the articular conditions generally, there existed a number of non-articular facts which have been claimed by various authorities in support of such a diagnosis. Thus Kent-Spender emphasized the diagnostic value of pigment spots in the skin; Garrod refers to the existence of psoriasis in such cases; the sense of stiffness and rigidity in the joints on waking in the morning is noted by Adams and Senator; and the last-mentioned author, as well as Garrod and other writers, remark upon the frequency with which rheumatoid arthritis appears after circumstances which cause much mental anxiety and depression. Undue rapidity of the pulse and a tendency to excessive sweating are also advanced by several authorities as features of the disease. In this connection it is interesting to note that Graves, in his "Clinical Medicine," alludes to certain cases of arthritic rheumatism attended with prolonged excitement of the circulation and copious sweating, and remarks that persons whose limbs are permanently stiff or distorted from rheumatic affections are generally found to have been suffering for years from this "sweating arthritis."

CASE 2.—Lizzie N., aged 43 (August, 1898), has had fair health except for "rheumatics." This commenced in her hands in 1886 and gradually extended to all her joints, so that four years ago she was practically a cripple and could hardly move from one room to another, a condition which persisted for two years but has gradually improved, until, at present, her gait appears quite easy and free from peculiarity. Now she only complains of occasional aching in her joints. She has never had rheumatic fever. Patient was married at 23, and has had twelve children, of whom six died in infancy from "bronchitis and convulsions"; the survivors enjoy good health; the youngest is aged five months. She has had one miscarriage, but has never suffered from any menstrual disturbance or disorder of the womb.

In the family history it is noted that patient's mother had deformed hands from "chronic rheumatism" and that she died of "consumption"; father killed in an accident; three sisters, all free from rheumatism, but subject more or less to bronchitis.

The condition of the hands is shown in the photograph (fig. 4), and it is only necessary to add that at the elbow- and knee-joints there was decided evidence of osteophytic formation. Movements of the knee-joints were free, but complete extension of the forearms was impossible on account of the enlargement of each olecranon.

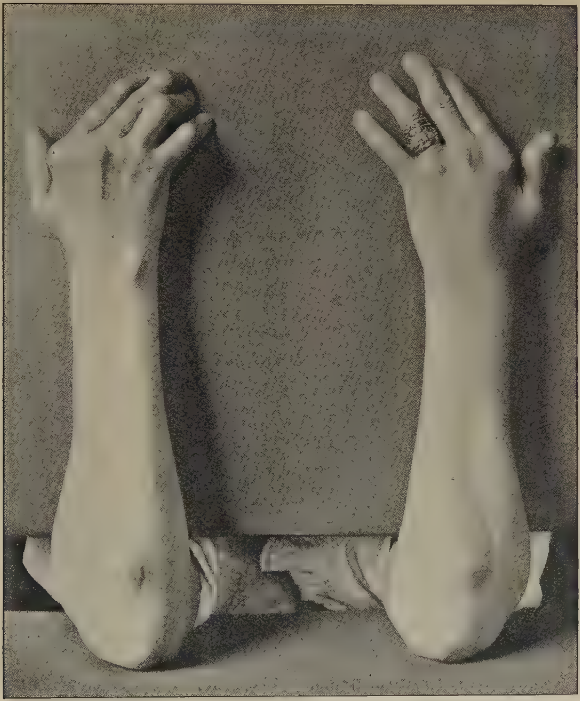

FIG. 4.—Rheumatoid arthritis with characteristic deformities of hands and atrophy of dorsal interossei, especially of left abductor indicis. Over each olecranon is seen a nodular subcutaneous tumour.

No cardiac disease, pulse about 80, tendon-jerks normal, no appreciable muscular atrophy except as seen in the hands.

The nodular mass present over each olecranon is entirely subcutaneous and is quite free from pain. Patient is sure that each tumour at one time was very much larger and was occasionally very painful. In the web between the finger and thumb in each hand is a small subcutaneous nodule.

In reference to the subcutaneous tumours, there is, beyond the fact of their existence, the interest of their diminution in size, this diminution coinciding with distinct recession of the joint symptoms. Of course, it is possible that these masses were originally the distended bursal sacs described by Adams, and that their diminution in size was the result of absorption of the fluid contents. Patient's impression is that they have always been hard, but her observations can scarcely be claimed as conclusive evidence. In Case 1, however, with a much more trustworthy patient, the history lent no support to the suggestion that the swelling over the olecranon was at any time a bursal sac, and the appearance of a second mass (p. 31) in the site of the excised tumour may be quoted against the suggestion that any of these olecranon tumours had a bursal origin.

Apart altogether from tumour formation, this case is well worthy of note on account of the undoubted improvement in the condition of the joints. Testimony is only too unanimous as to the sure, if slow, advance of rheumatoid arthritis, and therefore any justification, however slight, for lessening the gravity of the prognosis may well be welcomed. It must be observed that in this case there was not merely suspension of the advance of the disease, but, in addition, a very decided retreat of some of its worst symptoms. It is also noteworthy that this improvement took place during a period of active sexual life.

Case 3.—Chas. B., aged 63 (August, 1898), the subject of extreme deformities of the hands (fig. 5).

Patient was a strong and healthy man until his present trouble began some fifteen years ago. His first symptom was the sudden onset of severe pain in the ball of the right great toe, and he was told that this was an attack of gout. Pain and stiffness next appeared in the fingers and gradually spread to all the joints, though there is no history to suggest involvement of the intervertebral or temporo-maxillary articulations. On several occasions since the commencement of his illness he has had attacks distinguished by pain and swelling of various joints, the skin over the affected joints becoming "red and glazed." One of these attacks about ten years ago

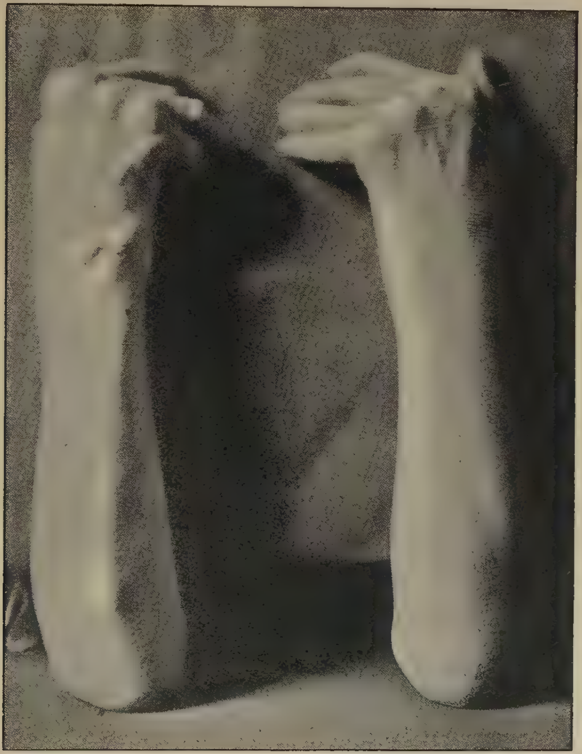

FIG. 5.—Extreme deformity of hands from osteo-arthritis. The prominence over the right olecranon is due, at least in part, to a firm subcutaneous tumour.

laid him up for six months, and was called "rheumatic fever."

He is married, and father of four living children; ten children died in early life. Has always been moderate in the use of alcohol. He appears to have a good knowledge of his family history, and denies that any of his relatives ever suffered from gout or any other form of joint disease.

The evidences of osteo-arthritis are seen more or less conspicuously in the hands, wrists, elbows, feet and knees. Further, there is some muscular wasting in the hands, the nails are longitudinally ridged, and the tendon-jerks both in the upper and lower limbs are very marked. There is no cardiac disease. The prominence over the olecranon (fig. 5) is partly due to periosteal thickening, but superficial to this, and freely movable in the subcutaneous tissue, is a firm nodular mass having exactly the same characters as seen in Case 2.

In the skin of each ear near to the helix and antihelix are two or three small tophaceous deposits, and patient says he has had "chalk-stones" removed from the skin over his great toe.

There is no doubt in this case of the existence of gout. The only question is whether the condition of osteo-arthritis is a consequence of the gout or whether, on the other hand, the co-existence of the two is a mere coincidence. Some authorities hold that osteo-arthritis is not a disease, but is a symptom which may appear as one expression of several different diseases, and here certainly the history of the case does seem to suggest that the original articular disturbance was gouty in character. Sir Dyce Duckworth, whilst allowing that both gout and rheumatoid arthritis may co-exist in the same patient, thinks that proof is needed before it can be admitted that gout, like rheumatism, may cause osteo-arthritis.

The feature of the case which brings it into relation to the present paper is the demonstration it affords, that, however produced, the phenomena of rheumatoid arthritis may include evidence of tumour formation in the subcutaneous tissue.

CASE 4.—Fred. H., aged 66 (May, 1899). Patient has been suffering from pains in his joints at frequent intervals for fully thirty years. The first part to suffer was the great toe, in which he has had many attacks of acute gout. On several occasions he has been confined to bed with pains in all the joints. His hands have been deformed for eight or ten years. His maternal grandfather suffered from gout, but he knows of no other member of his family who has had either gout or rheumatism. The evidence of gout in this case is

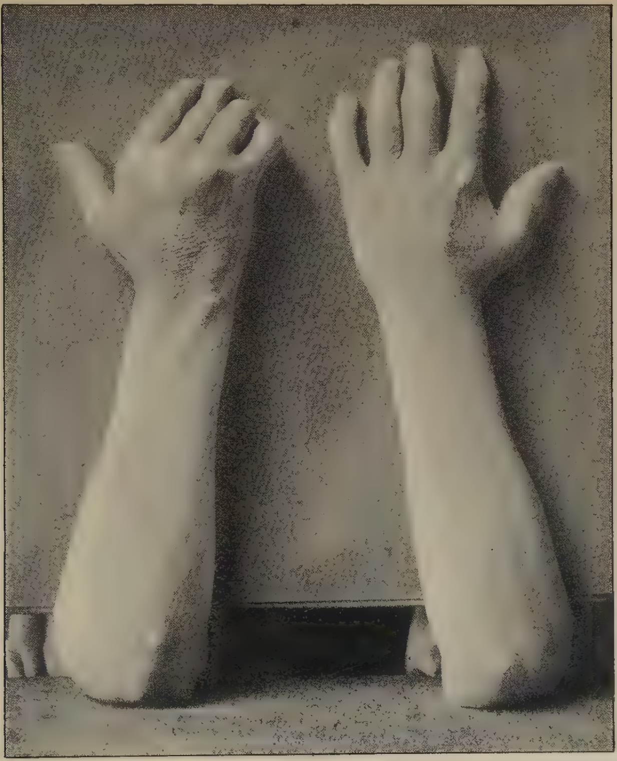

FIG. 6.—Photograph showing deformities of hands produced by gout and the presence over each olecranon of a firm subcutaneous tumour.

decisive, as numerous tophaceous deposits are present in each pinna and in the neighbourhood of the phalangeal joints of the fingers. The features of the case which are of special interest in the present connection are shown in fig. 6.

With regard to the condition of the hands it is to be noted that the appearances are not at all unlike the state of matters existing in rheumatoid arthritis, though there is much less symmetry than in the other cases here figured. The firm subcutaneous mass over each olecranon has exactly the characters of the tumours similarly situated in the other cases. Both of these facts have a manifest bearing on the question of the etiology of osteo-arthritis. If gout may not produce osteo-arthritis, it certainly produces osseous deformities of a very similar character (see also Case 3). And the present case proves conclusively that with such deformities there may exist subcutaneous tumours having exactly the same features as those found in rheumatoid arthritis. Why such tumours should specially develop over the olecranon is somewhat of a difficulty. But in none of the present series of cases is there any evidence to suggest that they originate from bursæ. Indeed, as already pointed out, the facts of Case 1 are strongly opposed to any such suggestion. Possibly the relation of the region concerned to the occurrence of external irritation is a factor of some moment.

Case 5.—Selina V., aged 67, widow (November, 1898). Patient came under observation on account of "failure of sight." The discovery of the conditions now to be described was quite accidental, or at least was not due to any complaint on her part. On several fingers of each hand there are very prominent Heberden's nodes, the most marked being on the middle finger of the left hand, and over these the skin is very decidedly reddened. Examination also detects at the base of the extensor aspect of the first metacarpal bone in each hand a considerable osseous thickening. This has an irregular nodulated surface and projects towards the carpus in a somewhat beak-like process, which seriously restricts extension of the metacarpal bone; passive movement of the joints produces some fine crepitus, but no coarse grating. Comparison of the conditions in the two hands shows a very high degree of symmetry. There is no definite deformity of the phalanges or metacarpus other than the above; muscular wasting, little or none; no pigmentation of the skin; nails normal. All the other joints seem free from evidences of disease.

The history of the state of her hands is, that about the time of the menopause, some twenty years ago, she began to be conscious of pains and stiffness in various joints, the shoulders and hands being, she thinks, the first affected. At one time her right hand was so stiff that she could not bend the fingers into the palm, and the whole limb was so weak that she could scarcely use it. She also had pains and stiffness in her knees but was never confined to bed. She remembers that for some time she was much troubled with pain and swelling "at the root of each thumb," and the skin here was "red and shiny." After this the "swelling of the bones" gradually appeared, and this has remained without apparent change for many years. The history of the development of Heberden's nodes is of the same chronic order, but has occurred without much pain; at times, however, the most prominent of these are, she says, very painful.

Patient had good health until the time of the menopause, but since then she has had a great deal of worry and her health has "broken down"; she looks, however, a fairly vigorous woman for her years. She married at twenty-nine, and during the succeeding twelve years gave birth to twelve children. Of these five died in infancy. The eldest of the survivors had rheumatic fever when a boy; no other rheumatic incidents. Patient's mother died of phthisis, but there is no known history of gout or rheumatism in the family.

The tumours which appear to bring the case within the present series have existed for many—between ten and twenty—years. They are in the subcutaneous tissue and are so much flattened that even the largest hardly catches the eye as a distinct projection. On passing the hand over the surface they are readily appreciated. They are firm, with, however, a distinct degree of elasticity, and are not very movable. They only cause complaint of pain on firm pressure. The skin over them is not reddened.

The tumours are situated as follows: In the left forearm there is one measuring about 1 in. in diameter in the upper third of the outer aspect, another of rather smaller size about the middle of the inner aspect, and a third, still smaller, in the front of the forearm some 3 in. above the wrist. In the right forearm there is a tumour similar in size and position to the one last named, and in both forearms there are here and there small nodules which can just be felt as slightly resistant points when the hand is passed over the surface. The last-described condition also exists over the extensor aspect of each thigh, but no larger nodules are present in the lower limbs. Patient says that at times she has pain in the larger tumours. The knee-jerks are distinct; cardiac sounds normal; urine free from albumin and sugar.

It will be observed that the tumours which are the special feature of this case have the characters of small lipomatous or fibro-lipomatous masses. Thus they exactly correspond to what has been described as one variety of the so-called rheumatic subcutaneous nodule (p. 20). Here, however, the tumours are associated with evidences of rheumatoid arthritis. The significance of such an association is suggested on p. 21.

SUPPLEMENTARY NOTES RELATIVE TO RHEUMATISM RHEUMATOID ARTHRITIS AND SUBCUTANEOUS NODULES.

On Heberden's Nodes.

It is not very infrequent to find that Heberden is credited with more definite views in relation to these nodes than he actually claimed. Except that he denied their occurrence in gout his attitude towards them was purely that of the open mind, and in reference to this denial it must be remembered that in his day gout was not charged with the subtle manifestations for which it is nowadays held responsible.

Heberden's short chapter on *Digitorum Nodi* is as follows:—

" What are those little knobs about the size of a small pea which are frequently seen upon the fingers, particularly a little below the top near the joint? They have no connection with the gout, being found in persons who never had it, they continue for life, and being hardly ever attended with pain or disposed to become sores, are rather unsightly than inconvenient, though they must be some little hindrance to the free use of the fingers."—" Commentaries," chap. xxviii, p. 148.

Charcot regarded the nodes as revealing " a constitutional state which is none other than the rheumatic diathesis." He says they are often accompanied by asthma, migraine, neuralgia, sciatica, and muscular rheumatism, and that it is not uncommon to meet with them in patients suffering from cancer of the breast or of some other organ. He agrees that they have no connection with gout, though they may co-exist with that disease.—*Op. cit.*, pp. 197-201.

A number of writers, as already mentioned, regard the nodes as a form of rheumatoid arthritis (p. 14); and several consider their development more frequent as an isolated event than in association with evidences of the disease in other parts.

James Begbie was very definite that " these little knobs . . . are the product of the gouty diathesis," though he allowed that they are often found in those who have never experienced a fit of the disease. He has seen them develop after " an inflammatory affection of the fingers more or less acute in its character, and attended by the same constitutional disturbance which marks the fit of gout; but more commonly they are the consequences of a slow and chronic disorder in which dyspeptic derangement has been chiefly noted." They are seldom, in Begbie's experience, found on "the hands of the industrious labourer or the hard-working mechanic."—"Contributions to Practical Medicine." 1862. pp. 21-29.

Sir Dyce Duckworth, on clinical grounds, concludes that whilst the nodes are not specially characteristic of either pure gout or pure rheumatism, they often, in women, co-exist with hemicrania, asthma, severe headaches, menorrhagia, and other troubles which are properly recognized as gouty manifestations of the sex. "They also occur in rheumatic arthritis and as a purely senile change."—*St. Bartholomew's Hospital Reports*, 1880, Vol. xvi, p. 190.

On Cardiac Disease in Rheumatoid Arthritis.

The statement has been confidently made that "rheumatoid" arthritis is never complicated by the occurrence of cardiac valvular disease, and on this reputed experience, at least in part, has been based the doctrine that whatever "rheumatoid" arthritis may be, it is certainly not a form of rheumatism. Before accepting this conclusion, however, it is necessary to inquire whether the clinical record on which it professes to rest is a complete and accurate one. Charcot, as already quoted (p. 9), did not allow it; on the contrary, he taught that "cardiac lesions are found pretty frequently in nodular rheumatism." At the same time, it may perhaps be admitted that most observers recognize it to be usual to find rheumatoid arthritis, seen as this is for the most part in adults and following a chronic course, unaccompanied by evidences of cardiac valve disease. But with this admission it is necessary to remember that even acute rheumatism causes valvular disease much less frequently in adults than it does in children; and, assuming the possibility of a chronic form of rheumatism, it might be expected that the escape of the adult from heart complications would in this be still more complete. Hence it is fair to say that the absence of cardiac lesions from the chronic rheumatoid arthritis of adults (even allowing this to be absolute) has a possible explanation other than that the disease is non-rheumatic.

The position just stated is strengthened when experience shows that in children and adolescents "rheumatoid" joint disease not infrequently has an acute or sub-acute character, and that in such cases cardiac complications are by no means unknown. In the illustrations which accompany this "Note" the joint conditions have distinctly the features of "rheumatoid" arthritis: it is the smaller and distal articulations which are affected; the hands (and in one case the feet also) display the characteristic deformities; the distribution of the disease in the individual patient shows a conspicuous bi-lateral symmetry; and, in addition, there is a considerable degree of muscular atrophy. Yet in every one of these cases there exists a lesion of the mitral valve—that is, a condition which is proclaimed to be "rheumatic" and *not* "rheumatoid." Each

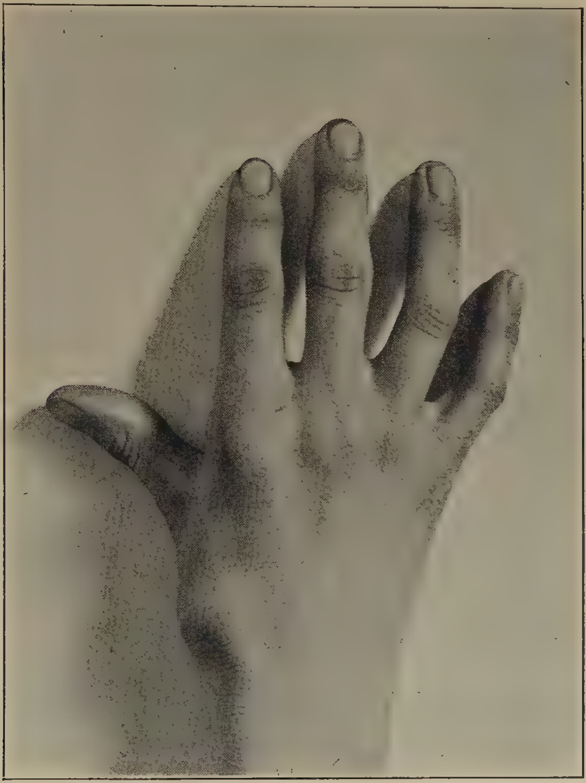

FIG. 7.—Showing enlargement of the heads of the second and third metacarpal bones, and the presence of a considerable thickening at the base of the first metacarpal bone. Patient, a woman aged 20, with a history of recurring pains in the hands and feet and the subject of mitral disease.

patient, in other words, exhibits one set of facts which, taken alone, are recognized as distinctive of rheumatoid arthritis, and a second set of facts which, viewed separately, are regarded as conclusive evidence of rheumatism. To say on the basis of such cases that rheumatoid arthritis is rheumatism would probably be claiming too much. But experiences of this order do certainly give pause to a ready adoption of the negative of this proposition.[1]

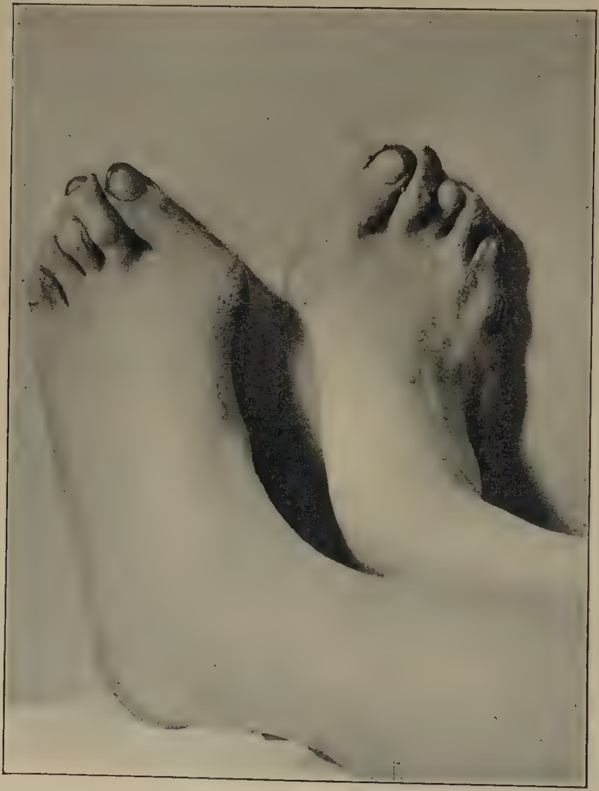

FIG. 8.—Showing enlargement of the heads of the metatarsal bones, and displacement outwards of their phalanges. (From the same patient as fig. 7.)

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[1] See also Dr. Hector Mackenzie's case, p. 23.

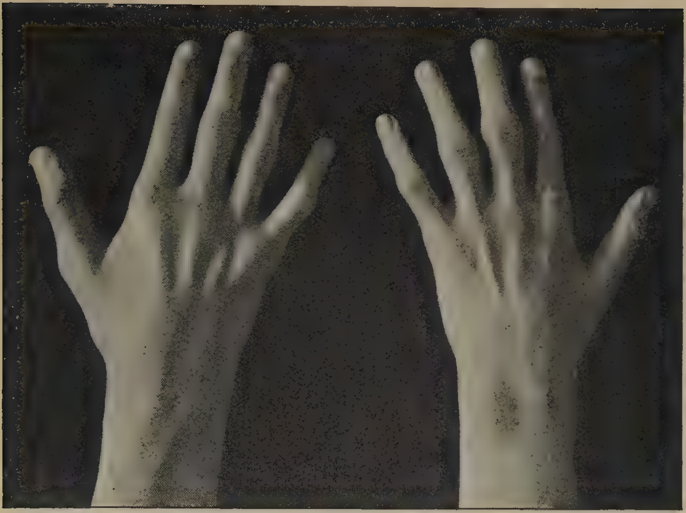

FIG. 9.—Hands showing fusiform swelling of interphalangeal joints, some enlargement of the heads of the metacarpal bones, and muscular atrophy. Patient a girl aged 18, and the subject of mitral disease.

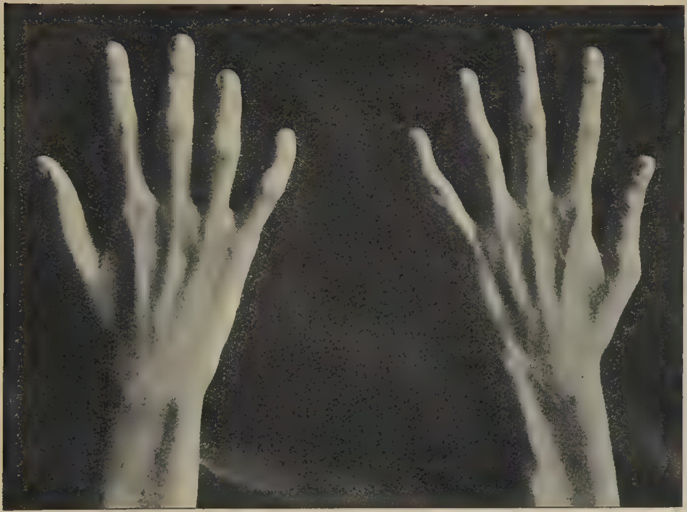

FIG. 10.—Hands showing marked muscular wasting (atrophy also of extensors in forearms), some enlargement of the heads of the metacarpals, and a degree of swelling of the interphalangeal joints. Patient a girl aged 15, and the subject of mitral disease.

The debate between the rheumatic and non-rheumatic nature of rheumatoid arthritis means essentially, Are the articular conditions in rheumatoid arthritis and in the acute polyarthritis of rheumatic fever due to the same cause, or to different causes? When this question can be answered, but hardly before, the solution of the problem will be obtained. In the meantime, of causes we know nothing certain; and a sure conclusion is therefore impossible. All that is claimed for the experiences here exhibited (figs. 7, 8, 9, 10) is that they suggest the need for caution before hurriedly accepting the current doctrine that between rheumatism on the one hand, and rheumatoid arthritis on the other, there exist distinctions which are clear, certain, and free from qualification.

On Muscular Atrophies in Rheumatoid Arthritis.

The occurrence of muscular atrophies and exaggerated tendon-jerks in certain cases of rheumatoid arthritis is sometimes urged in support of the suggestion that the articular lesions of this disease are essentially dystrophic changes—the results of disturbances in the spinal cord, either primary in origin, or caused by peripheral irritation in the pelvic or other viscera. But admitting the truth of the events, and admitting that they indicate spinal disturbance, the conclusion suggested does not follow. For, it has long been known that any arthritic lesion may produce muscular wasting, and that, like the wasting in rheumatoid arthritis, this may be too extreme and too rapid in development to be the result of mere disuse. Sir James Paget, many years ago, in treating of the arthritic complaints met with in hysterical patients, advanced the proposition that "if a joint has long been very painful, and yet there is no wasting of the muscles near it, it is not inflamed." He directed attention also to the muscular atrophies which may follow injuries to the joints; taught that such atrophies were not due to non-use of the muscles; and suggested their causation in the "disturbance of some nutritive nervous centre irritated by the painful state of sensitive nervous structures." Charcot maintained similar views. He also pointed out that not only is the arthropathy the primary fact, but further, that there is no necessary relation between the intensity of the joint affection and the atrophic phenomena, "a slight and easily cured sprain, a simple collection of fluid in a joint, non-inflammatory and non-painful" being quite sufficient to cause even extreme atrophy. The text of his lecture on this subject, indeed, is an individual case in which a slight injury of the knee was followed by great wasting

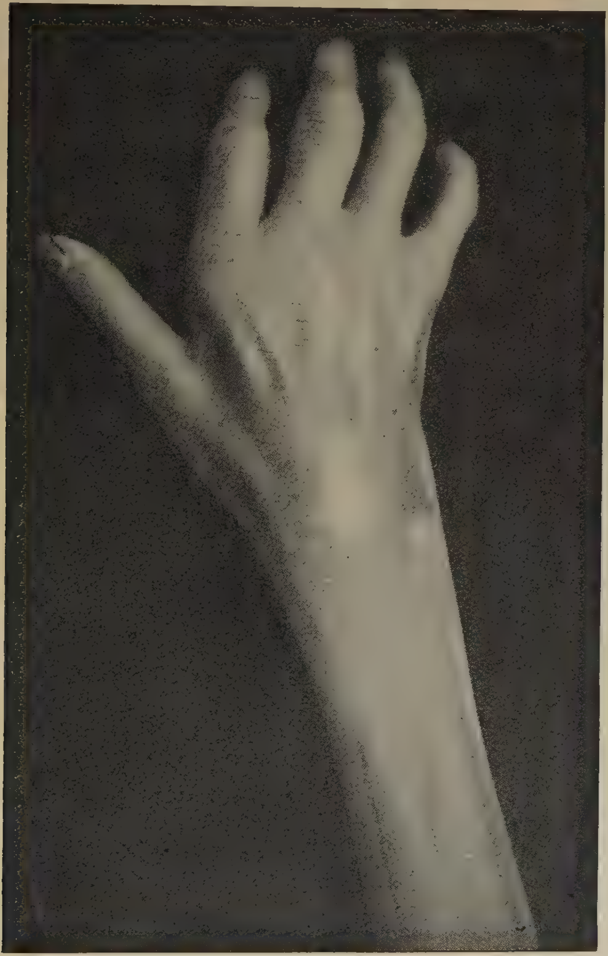

FIG. 11.—Rheumatoid Arthritis, with wasting of muscles of hand, especially of abductor indicis. (From a photograph by Dr. John Reid.)

of the extensors of the joint. Charcot also showed that these arthritic muscular atrophies have a special tendency to display themselves in the extensor muscles—a conclusive argument against their causation being a mere disuse of the part. Again, it has been shown by Judson Bury that acute rheumatism provides a certain percentage of cases with marked muscular

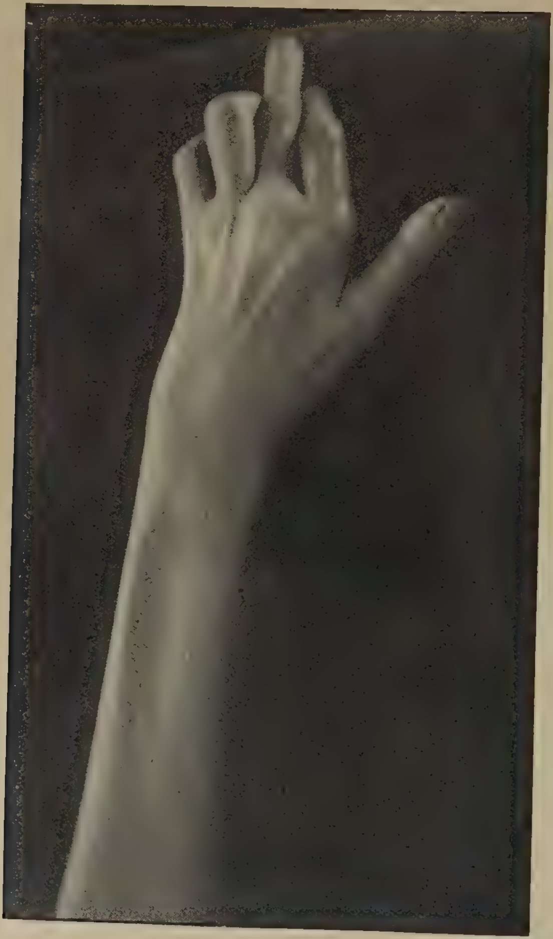

FIG. 12.--Rheumatoid Arthritis, showing deformities of fingers, and atrophy of dorsal interossei and of extensors in forearm. (From a photograph by Dr. John Reid.)

atrophy, and that, excluding those dependent upon a peripheral neuritis, a definite proportion remains to support Charcot's teaching. If further proof is needed, it is provided by the muscular atrophies which have promptly followed attacks of synovitis artificially induced in dogs by the injection of solution of ammonia into the synovial cavities.

The natural conclusion, therefore, in reference to the muscular atrophies of rheumatoid arthritis, is, that, like the atrophies which accompany other affections of the joints, they are secondary to the joint lesions, which probably produce them through a reflex mechanism. The only observation which can be set against this conclusion is the claim that in certain cases the muscular wasting has preceded the changes in the joints. This, of course, means the *recognized* changes in the joints; but, remembering how insidious may be the pathological process, and how, as in the instances given by Charcot, a slight disturbance in the joint may determine a high degree of muscular atrophy, this exceptional experience cannot be held to justify an attempt to place the muscular atrophies of rheumatoid arthritis on a different platform from the atrophies which accompany other diseased conditions of joints.

The increased tendon-jerks are in much the same position, for, when muscular atrophy follows a disease of, or injury to, a joint, there is usually at the same time evidence of increased myotatic irritability.

The argument which can be derived from the existence of muscular atrophies and increase of the tendon-jerks in rheumatoid arthritis cannot be carried in favour of the dystrophic explanation of the disease beyond this point: It is admitted that peripheral disturbances in the joints may produce reflexly muscular wasting and exaggeration of the tendon-jerks, therefore it must be allowed as possible that peripheral irritation in other organs (*e.g.*, the uterus) may cause a like result; and again, if pathological changes in the spinal cord may lead to disturbances in the nutrition of muscles, they may also surely lead to changes in the nutrition of the joints—a contention which is supported by the arthropathies known to accompany certain recognized central nervous lesions.

All this may be admitted, but the point which it is now desired to emphasize is, that the mere fact of the existence of muscular atrophies and exaggerated tendon-jerks in rheumatoid arthritis is no proof that the *articular conditions* are dependent on a spinal lesion. It is equally in harmony with the facts to hold that the articular changes are the primary events, and that the muscular wasting and the exalted tendon phenomena are secondary consequences produced reflexly by the joint irritation.

ON SUBCUTANEOUS NODULES IN CASES FREE FROM BOTH RHEUMATIC AND RHEUMATOID JOINT DISEASE.

Occasional cases have been recorded in which subcutaneous fibrous tumours have been present, although evidences of rheumatic or rheumatoid changes in the joints have been wanting. These cases present so wide a variety that it is difficult to find any basis on which they can be classified. They have this relation to the preceding essay—that their occurrence tends to negative the doctrine of the rheumatic significance of subcutaneous fibrous tumour formation. But they also weaken the argument that the occurrence of such tumours in rheumatoid arthritis suggests that this disease is essentially a form of rheumatism. The cases, however, exist as clinical facts, and they have an obvious bearing on the present discussion. A number of the records, in brief, are therefore reproduced here as a supplementary note.

(1) (Sir Stephen Mackenzie.) The patient, a woman aged 40, was the subject of secondary syphilis; there were several subcutaneous nodules having physical characters identical with those described by Sir Thos. Barlow and Dr. Warner; some of the tumours had persisted for two years. No personal or family history of rheumatism; and no arthritis.[1]

(2) (Sir Dyce Duckworth.) A woman, aged 38, who had probably had secondary syphilis. The nodules, which were numerous, were present in the skin, subcutaneous tissue, and periosteum; they had existed for nearly three years. A distinct family history of rheumatism, but no personal rheumatic incidents.[2]

(3) (Sir Kingston Fowler.) A man, aged 35, with a history of a venereal sore, but no recognized secondary symptoms, and no personal or family suggestion of rheumatism. Numerous nodules, symmetrically arranged, were present over the bony prominences of the limbs; some of them spontaneously disappeared, to reappear later. Nodules shown to consist of dense fibrous tissue with blood-vessels having "thickened muscular coats."[3]

(4) (Sir Dyce Duckworth.) A woman, aged 32, the subject of chronic lead poisoning and acute gout, in whom there appeared a number of subcutaneous nodules over each tibia.[4]

(5) (Carver.) Two cases of nodules in palms and flexor aspects of fingers. Patients—men over fifty—described as

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[1] *Transactions of the Clinical Society*, Vol. xvi, p. 188.
[2] *Ibid.*, p. 190.
[3] *Ibid.*, Vol. xvii, p. 65.
[4] *Ibid.*, Vol. xx, p. 266.

"rheumatic, dyspeptic, and somewhat gouty." Nodules disappeared under treatment.[1]

(6) (Sir Jonathan Hutchinson.) A man, the subject of inherited gout, with periosteal outgrowths, bursal enlargements, and thickening of tendons.[2]

(7) (F. R. Walters.) A male adult, who, after influenza followed by double pleurisy, developed numerous nodules about the finger-joints, with thickenings of the palmar fascia. The nodules here are named "rheumatic," and the first cardiac sound is described as harsh and re-duplicated at the apex.[3]

Dr. Gilbert Bannatyne and the late Dr. W. Washbourn have also described the presence of subcutaneous nodules in cases of influenza, and the former, in a single instance, has seen such nodules in gonorrhœal arthritis.

(8) (R. T. Godlee.) Male patient, aged 27. No personal or family history of rheumatism; no syphilis; scars in neck and on cornea. Complained of a "peculiar swelling of the nose and eyelids," and developed numerous fibrous nodules over forehead, back of hands and sacrum. The bursa over the olecranon was enlarged.[4]

(9) (H. G. Turner.) A man, aged 45, who was the subject of a firm, non-œdematous swelling above and below the eyes, and complained of numbness and tingling about the finger tips, had numerous nodules over the head and upper extremities, some superficial, others attached to the periosteum; none related to tendons or nerve trunks. No history of either rheumatism or syphilis.[5]

(10) The late Dr. George Carpenter described and figured two cases of multiple subcutaneous nodules in infants free from other evidences of rheumatic disease.[6]

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[1] *Lancet*, 1888, Vol. i, p. 475.

[2] *Transactions of the Clinical Society*, Vol. xxii, p. 241.

[3] *Ibid.*, Vol. xxv, p. 241.

[4] *Ibid.*, Vol. xxvii, p. 272.

[5] *Ibid.*, Vol. xxx, p. 218.

[6] *Transactions of the Society for Study of Disease in Children.* Vols. i & viii.

ON THE OCCURRENCE OF RHEUMATIC FEVER WITHOUT ARTHRITIS.

THE existence of definite and more or less prolonged febrile disturbance, without the discovery of signs or symptoms adequate to account for such disturbance, is a diagnostic problem of not infrequent occurrence. The difficulties of the problem become all the greater when, in consequence of the lapse of time and the non-appearance of any distinguishing skin eruption, the possibility of the development of any of the specific febrile diseases has been ruled out of court. Even an advance to this position is not always easy, at least in relation to enteric fever. Certainly no case with the features here suggested but carries with it the possibility of such a diagnosis. Tuberculosis is, of course, another condition which, in the same circumstances, occurs to the mind, and whilst the definite establishment of either diagnosis may well be impossible until more positive evidence emerges, the negation of either diagnosis, so long as pyrexia persists, is often a quite unwarranted conclusion. There are also other diseases which may present themselves mainly in the form of disturbed temperature and whose prominence in this respect is perhaps less generally recognized. Pernicious anæmia, leucocythæmia, and certain allied clinical conditions, may undoubtedly be the cause of very decided and prolonged febrile disturbance, and there is danger here of regarding the weak and exhausted condition of the patient as merely the result of the pyrexia, and of thus missing the hint which the general state affords. By an examination of the blood, and the use of the ophthalmoscope, an obscure case of high temperature may, at least occasionally, be promptly brought to the point of an exact diagnosis [1].

The question may be asked whether there are any circumstances other than those above enumerated which will account for the existence of fever without explanatory physical signs, and the object of this paper is to suggest that rheumatism is such a condition. At first sight that suggestion seems a somewhat extraordinary one. Experience so frequently shows the existence in acute rheumatism of a fairly close parallelism between the extent and severity of the polyarthritis and the range of the febrile movement; and so usual is it to note the associated subsidence of the two series of events, with, not uncommonly, their subsequent return together, that one might well be excused, when the question is raised, for answering off-hand, as it were, that the febrile disturbance in rheumatic fever depends upon and is determined by the severity of the arthritis, it being an undoubted truth that a local inflammatory condition frequently is responsible for upsetting the normal temperature equilibrium. But merely a moment's consideration is sufficient to show the necessity for admitting that even if the degree of the polyarthritis and the height of the pyrexial curve afford an absolute correspondence, it is still as reasonable to regard them as dependent upon a common cause, and equally responsive to this cause, as to hold that one is the result and issue of the other. The simple fact that two series of events execute their movements *pari passu* is of course no proof that their relations are those of cause and effect. And taking the above statements in reference to acute rheumatism as absolutely accurate, it may as justifiably be argued that the severity of the fever determines the intensity and distribution of the synovitis, as that the acuteness of the synovitis is the cause and measure of the fever. Either one or other proposition can be stated in harmony with the facts as now accepted, and though general experience inclines us to rest an interpretation of pyrexia upon local inflammation when this exists, the very opposite thesis in relation to rheumatic fever was maintained some years ago by no less an authority than Dr. Budd [2]. As he defined the position, " it is the fever chiefly which tends to extend the rheumatic inflammation over a great number of parts and thereby increases the liability of the heart, in common with other parts, to become affected with rheumatic inflammation."

Conjectures and arguments may be advanced on either side. Thus it is no doubt true that in rheumatic fever there is often a close correspondence between the amount of the local articular disturbance and the febrile level; that in children, in whom non-articular rheumatism is the prevailing mood, decided pyrexia is uncommon; and that, unlike the course of the temperature in the specific fevers, pneumonia, etc., the chart of acute rheumatism does not lend itself to be displayed as conforming to a definite plan or type. On the other hand, it may be remarked that the symptoms of febrile invasion often, or indeed generally, precede the local evidences of acute rheumatism; that the escape of the temperature mechanism in childhood is scarcely more remarkable than the escape of the synovial membranes; and that even the absence of a " type " is no conclusive proof that the fever of acute rheumatism is merely an " accident " attached to the synovitis.

However ingeniously these arguments may be debated, the appeal in the end and for practical purposes must be to clinical facts. To the question can rheumatism be a possible explanation of pyrexia existing apart from local phenomena, it is actual experience must supply an answer; and whilst any individual incident may in itself have but small probative value in supporting an affirmative response, the multiplication of such incidents may justify a presumption sufficiently strong to determine the views and actions of reasonable men. An individual experience which points in this direction will now be quoted :—

Mary C., aged 18, was sent into hospital on October 1, 1897. She had for several days been complaining of headache and of aching in the back, with thirst and want of appetite. On admission she said that her throat was sore, and that her back and legs ached; the temperature

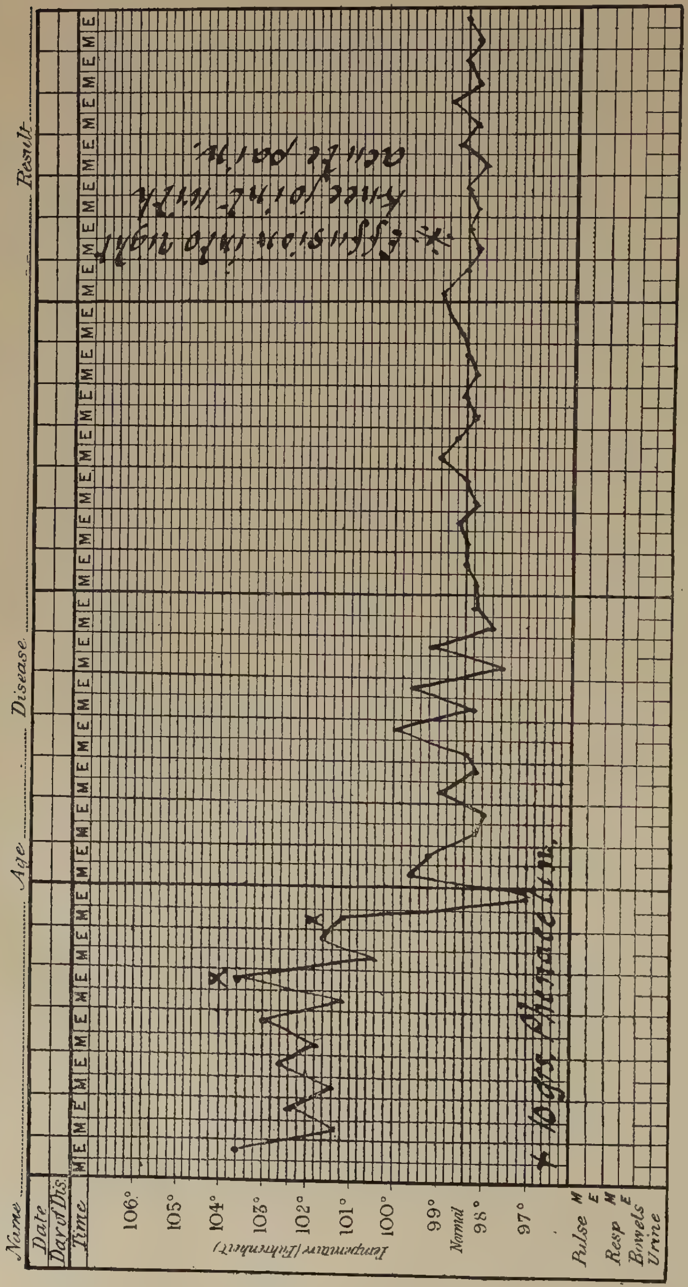

FIG. 13.—Period of unexplained pyrexia followed after an apyrexial interval by an acute (? rheumatic) synovitis of the right knee-joint.

was 103.6°. Examination showed slight congestion of the fauces and some tenderness on pressure over the knees and shins, but no redness or swelling of the joints, no skin eruption, no evidence of disease in the thoracic or abdominal viscera, and no history or existence of vaginal discharge. Patient was a well-nourished girl with little or no evidence of anæmia, and the fundus oculi on each side was normal. The febrile temperatures continued until October 8, as shown on the accompanying chart, and during the whole of this time she always said she was "quite well," though under pressure she admitted some aching in her legs; the skin was always more or less moist, but there was no free sweating. The tongue remained clean; the bowels were constipated; and neither in the character of the stools nor from any other source was there anything to suggest the existence of enteric fever. On October 9 the temperature fell to subnormal, and the case pursued an uneventful course until the 23rd. On that day the patient complained of acute pain in the right knee, and the joint became distended with fluid. Under rest this condition promptly subsided, and patient left the hospital quite well.

In the study of this case it is manifest that during the febrile period the elements for the construction of a positive diagnosis did not exist. Such grounds of complaint as the patient offered were but slight, and quite out of proportion to the height of the fever. Physical examination was equally helpless to throw light on the clinical condition, and it seemed probable, even for some days after the termination of the fever, that the case would have to be labelled "unexplained pyrexia"; unless, without a shred or fragment of positive evidence, one's courage could claim a diagnosis of "abortive typhoid."

The question now to be considered is whether the subsequent development of acute synovitis in the knee-joint justifies a diagnosis of rheumatic pyrexia. Certainly had the two events—the pyrexia and the synovitis—coincided in point of time, the only difficulty in naming the case one of "rheumatic fever" would have been the restriction of the inflammation to a single joint. It may be doubted whether such a restriction is a valid argument against the diagnosis of acute rheumatism, and the sudden appearance of the synovitis, and its prompt and complete disappearance, are exactly on the lines of "rheumatic" inflammation. Besides, in the survey of the case it is necessary to include the whole of the phenomena, and though the absence of any other explanation is not a deciding argument, it may at least be urged that a diagnosis of rheumatism covers the entire ground—the pyrexia, the early symptoms, and the later synovitis. And support for this view may be claimed from not unimportant sources. For, though most modern authors, in dealing with this subject, are content with the general statement that the pyrexia in acute rheumatism in the great majority of cases corresponds to the extent and severity of the synovitis, earlier writers of recognized authority, whilst adhering to the truth of the rule, were much more explicit, that exceptions occurred, and that it was quite possible to have "rheumatic fever without arthritis." Of course the difficulty of establishing this last proposition arises from the dependence of the recognition of rheumatism upon certain local events, and in the instance related above, had it not been for the development of synovitis in one of the joints, the case would have been free from individual features which could be claimed as sanctions for a diagnosis of "rheumatic pyrexia." It is the development of this local condition at a late date which makes the record of the case peculiarly significant as an illustration of the possibility of a complete divorce between the constitutional disturbance and the local phenomena of acute rheumatism.

Graves [3] seems to have been the first authority to announce this doctrine. He observed in several instances that patients who at an earlier date had suffered from articular rheumatism developed "attacks of fever which in intensity, duration, and every other particular, were identical with their former fevers, save and except that from beginning to end not a single joint was inflamed." On the basis of this experience he maintains the proposition that "as arthritis may exist without rheumatic fever, so rheumatic fever may exist without arthritis."

Todd [4], also, deals with this question, and referring to the cases related by Graves says: "I have not met with such cases, but I have frequently been struck with the disproportion of the intensity of the articular affection to that of the fever, the former being trifling in amount, whilst the latter was of great intensity." He holds to the view that the articular swellings and the febrile movement are the result of a common cause, viz., the presence in the blood of a special morbid element.

Sir Thomas Watson [5] was of opinion that rheumatic fever "may sometimes run its whole course without any manifest affection of the joints."

Taylor [6], in a most interesting paper published in 1845, in arguing, from cases quoted in detail, for the occurrence of rheumatic pericarditis without inflammation of the joints, remarks, "This opinion implies the possibility of rheumatic fever without arthritis, and is supported by the facts which support the latter opinion."

Fuller [7], also, upholds this view: "in certain instances," he says, "it (the rheumatic poison) may excite the peculiar train of symptoms whereby rheumatic fever is characterized, without producing from first to last the slightest concurrent local inflammation, whether of the joints, or of the heart, or of any other organ."

Charcot [8], too, though less confident on the point, concludes that on the whole experience seems to justify the view of Graves, Todd, and Fuller, that the fever in rheumatism is primary and not secondary.

And more recently Gee [9] has written on the same subject and has described cases which support the same doctrine.

REFERENCES.

[1] *Clinical Journal*, Vol. x, p. 133.
[2] *Medico-Chirurgical Transactions*, Vol. xxviii, quoted by Dr. John Taylor.
[3] "Clinical Medicine." Vol. ii, p. 182.

[4] "Practical Remarks on Gout, Rheumatic Fever, and Chronic Rheumatism of the Joints." p. 125.
[5] "The Principles and Practice of Physic." Fourth Edition, p. 740.
[6] *Medico-Chirurgical Transactions*, Vol. xxviii, p. 520.
[7] "On Rheumatism, Rheumatic Gout, and Sciatica." Third Edition, p. 30.
[8] "Lectures on Senile Diseases." p. 158.
[9] *St. Bartholomew's Hospital Reports*, 1888, Vol. xxiv, p. 21.

THE PREVENTION OF CARDIAC DISEASE IN RHEUMATIC CHILDREN.[1]

THE note which I now submit to the Society concerns itself with this essentially practical question, Can anything be done to prevent or to limit the occurrence of cardiac disease in children who are known to be liable to such disease as the result of a rheumatic proclivity or inheritance? In a Society specially concerned with the clinical and pathological misfortunes of childhood it needs no presentation of figures to secure a recognition of the appalling frequency with which valvular and other forms of organic heart disease find their victims among those of tender years. The repetition of such a discovery in individual patients is a constantly recurring experience, and this in private practice not less than in the out-patient departments and wards of our children's hospitals. Nor is it necessary to picture the consequences which attend this grave disease. A life of limited activity and diminished usefulness is the least of its almost inevitable penalties. Further, the acuteness and pathos of the situation are frequently heightened by the statement, often conveying a note of protest, that the child's life-history has been almost or altogether free from any incident calculated to awaken suspicion in the minds of the parents that serious mischief was afoot. Some relatively slight febrile illness, or a local disturbance of apparently trivial import, may be admitted, but surely these, it is urged, cannot have meant something so serious as the risk of organic heart disease.

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[1] Read before the Society for the Study of Disease in Children, January, 18, 1907.

Such is the argument that not unnaturally seems valid to the general public, to whom the rheumatism which means the risk of heart disease, is a more or less acute illness accompanied by pain and swelling of the joints. The medical profession, however, is compelled to an entirely different verdict, namely, that irreparable damage is frequently inflicted on the cardiac tissues by the rheumatic poison, almost or altogether apart from incidents popularly recognized as rheumatic.

Can anything be done to save or to modify so disastrous a situation ? Now, thanks to modern teaching, more particularly that of the last twenty-five years or so, the profession generally has come to recognize that rheumatism in childhood presents a much more varied symptomatology than does the same disease in adults. While shutting out from the category of rheumatism many joint conditions which were formerly so labelled, medical opinion in modern days has extended the borders of the disease so as to include a number of events, the rheumatic origin and nature of which were previously unsuspected. And it is in the early years of life more especially that these events occur. Chorea, erythema, tonsillitis, some forms of pleurisy, subcutaneous nodules, slight pains in the joints or other parts, not to mention more dubious occurrences, are all, to the medical judgment—at least in frequent instances—manifestations of the disease which, in adults, appears as, for the most part, an acute polyarthritis. And certainly with these forms of rheumatism there is, not less, probably indeed much more, than with the rheumatic fever of the adult, a definite risk of cardiac disease. In brief, the general attitude of professional opinion towards rheumatism is, that in the adult the disease expresses itself largely as an acute polyarthritis, whilst in the child it occurs mainly in non-arthritic forms ; and that with each and all of these, and more especially with the non-arthritic manifestations, rheumatic heart disease is a possible incident.

Now this disproportionate frequency of heart disease in rheumatic children, as compared with adults suffering from the influence of the same poison, may reasonably be regarded as associated with a difference which obtains between the two contrasted conditions in relation to the practice of rest. Everyone admits that in the present state of our knowledge, it is to early, to complete, and to sustained rest we have to look as the most efficient means of preventing the development of cardiac complications in the course of a rheumatic attack. Such a discipline in the actively rheumatic adult is imposed, almost inevitably, by the severity and site of the pain. The patient is driven to bed, and to remain there, because the condition of his joints leaves him no option. But the rheumatic child, even during an active phase of his disease, often suffers little or no pain, and thus circumstances permit him to escape the rest which an acute polyarthritis invariably compels. Is it not probable that in this difference is to be found, at least in a large degree, an explanation of the relative frequency of heart disease in early life? Let this be granted, and it at once becomes obvious that the protection of these children can only be accomplished by the provision of an arrangement which will secure for them not less than the same measure of rest which the disease itself enforces in the acute polyarthritis of the adult. But no such end is possible so long as the public is unaware of the fact that rheumatism in the child is often, or even usually, a disease which avoids the joints, and occurs mainly in the shape of various non-arthritic disturbances, which, to the uninstructed, frequently appear altogether trivial and unimportant. If the protective influence of rest is to be obtained during such disturbances, the parents or guardians of rheumatic children must be made aware of their true nature, and of the serious risk in the shape of heart disease which they carry. In other words, the public, or at least those members of the public who are responsible for the care of children having a rheumatic inheritance, must be educated to appreciate the clinical characters which rheumatism assumes in childhood, the special danger to the heart which these involve, and the urgent necessity for early, complete, and prolonged rest, if such danger is to be avoided or minimized. Until this position is attained, these unfortunate children will continue to be deprived of the one influence by which they may possibly be saved from supreme disaster. But such a position cannot be attained without the direct and active intervention of the medical profession, with whom rest both knowledge and opportunity. With the profession, therefore, rests also responsibility.

If all this is true, it follows that the discovery of rheumatic manifestations in any patient demands from the medical practitioner immediately concerned the discharge of a much wider responsibility than the care of the individual sufferer. And particularly does this responsibility extend to those members of the family in whom, on account of their age, rheumatism is prone to occur in its more subtle, and therefore, so far as the heart is concerned, its more dangerous forms.

To put the position in specific terms, the discovery of signs or evidences of rheumatic disease in any family ought to be the occasion, as an imperative professional duty, to convey to the parents information and warning of the special risks which even the apparently slight illnesses of their children may involve, and of the importance of complete rest as a means of avoiding these risks. Let this be done regularly and systematically by every member of the profession on the first discovery in any family of signs of a rheumatic inheritance, and there will be some chance of gaining for young children the protective influence of rest during the apparently slight, but essentially dangerous, manifestations of rheumatism which are common in early life. And with this, it is reasonable to believe, there would be some diminution, and possibly a considerable diminution, in the incidence of organic heart disease, at least in early life. What the future may have in the shape of prophylaxis or treatment we do not know. At present the one influence upon which we depend for the prevention of heart disease in rheumatism is absolute rest so long as the rheumatic poison shows signs of activity. The benefit of this influence ought to be secured for rheumatic children. But it can only be secured by the deliberate adoption by the entire body of the profession of the policy here advocated. This policy ought, therefore, to be accepted as an imperative command and universal obligation—the one opportunity which exists, amidst our present ignorance, of protecting rheumatic children from the profound misfortune of organic cardiac disease.

ON ARTHRITIS ACCOMPANYING OPHTHALMIA NEONATORUM.[1]

For a recognition of the truth that the purulent ophthalmia of newly-born infants may be associated with joint complications analogous to those which not infrequently attend urethritis, we are indebted to Mr. R. Clement Lucas.[2] His first observation, made so far back as 1885, was, perhaps not unnaturally, subjected to the ordeal of a somewhat incredulous criticism. From this, however, it emerged successfully, and the suggestions that his case might have been one of epiphysitis or of syphilitic synovitis were successfully met by clinical arguments that can hardly be gainsaid. The lapse of time has provided further evidence. This is by no means abundant so far as volume is concerned, but it is supplied by a number of independent observers and contains elements which raise it to the level of established proof.

To judge from systematic works dealing with the subject of ophthalmia neonatorum it would appear that Mr. Lucas's original observation hardly attracted the attention which it deserved, though for this some excuse may perhaps be found in the experience that, with one exception, described by Mr. R. G. Fendick,[3] also in 1885, no parallel case was recorded in Great Britain during a period of thirteen years. And when Mr. Lucas in 1899 introduced the subject to the Royal Medical and Chirurgical Society a search through

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[1] A paper read before the Society for the Study of Disease in Children on May 16, 1902.

[2] *British Medical Journal*, 1885, Vol. i, p. 429: Vol. ii, pp. 57, 699, 773. *Transactions of the Royal Medical and Chirurgical Society*, Vol. lxxxii, p. 137.

[3] *British Medical Journal*. 1885, Vol. ii, p. 830.

European medical literature had resulted in the discovery of only twenty-three cases. Writers on gonorrhœal rheumatism subsequent to Mr. Lucas's later paper began, however, to recognize the occasional occurrence of joint complications in ophthalmia neonatorum, and the association was mentioned by Dr. Archibald E. Garrod[1] in 1890, and by Dr. J. Mitchell Bruce[2] in 1894. More recently Mr. Sydney Stephenson[3] has given some interesting particulars of the character of the joint affections, and Dr. L. Vernon-Jones[4] has also drawn attention to them. Yet, from what has been stated above, the cases, manifestly, are anything but numerous, and so it seems desirable that examples of the condition should be put upon record, more particularly as in some instances the subacute character of the inflammation and its temporary duration may cause it to be overlooked.

The example now to be reported can only claim the interest that it confirms the results of the earlier observations, more especially as regards the possibly quiet nature of the arthritis, and the prompt and thorough restoration of the efficiency of the affected joints. The diagnosis in this individual case was not confirmed by bacteriological examination, though this has been accomplished in other instances and in reference both to the conjunctival discharge and the synovial effusion. But as we have medical warrant that the father of the patient had gonorrhœa two months before the child was born, that the mother was subsequently affected, and that the child began to suffer from purulent ophthalmia when two days old, the diagnosis is not open to doubt.

The earlier particulars of the case, for which I am indebted to Dr. John Clarke, of Plumstead, are as follows. The affection of the eyes commenced two days after birth, and an abundant purulent discharge continued for fully three weeks. When the child was a fortnight old, the right shoulder-joint became considerably swollen, and the right wrist less so, without apparently causing the child much discomfort; and the swelling subsided in the course of a fortnight without any very active treatment. When the child came under my observation he was six weeks old. There was still a slight amount of muco-purulent discharge from the conjunctivæ, and the right cornea presented a small abrasion from which a diffuse haze spread through the corneal substance. It may perhaps be conjectured that here is to be found the explanation of a further outbreak of joint inflammation. For, while the joints which were first attacked—that is, the right shoulder and wrist—had completely regained their normal characters, the left elbow-joint was much enlarged and the swelling extended down the radial side of the forearm for a considerable distance, giving here a sensation of deep fluctuation, as though the sheaths of the muscles were involved. The skin was not reddened, and though the child objected to manipulation of the joint he appeared otherwise to be free from pain, and his general nutrition was entirely satisfactory. It was decided to fix the limb and to continue the local treatment of the ophthalmia, and under these measures the patient made a rapid recovery, so that in from two to three weeks the elbow-joint and forearm had regained in every respect their normal characters.

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[1] "Treatise on Rheumatism." p. 185 (Charles Griffin and Co.).

[2] "Quain's Dictionary of Medicine." Second Edition. Vol. ii, p. 681.

[3] "Contagious Ophthalmia." 1890 (Baillière, Tindall and Cox).

[4] "Gonorrhœal Arthritis." 1891 (H. K. Lewis).

Attention may first be drawn to the subacute character of the successive attacks of arthritis. Almost from the outset of his observations Mr. Clement Lucas recognized that two varieties of arthritis associated with ophthalmia neonatorum occurred—viz., (1) a very acute form with features suggesting a tendency to suppuration, and (2) a milder form accompanied by a great deal of effusion and by pain on movement, but with little or no surface redness. The present case adapts itself exactly to this latter form.

A second feature worthy of note is the extension of the swelling beyond the limits of the joint cavity. This is, of course, a well-recognized event in many cases of arthritis of urethral origin, and Mr. Sydney Stephenson has drawn attention to "effusion into the sheaths of the tendons" in the neighbourhood of the affected joints in cases where the primary gonococcal inflammation involves the conjunctiva.

The joints most commonly affected would appear to be the knees and the wrists, but in the present case the knees escaped altogether and the stress of the disease fell on the right shoulder and left elbow. It may be added that no history of injury could be obtained and there was no evidence of syphilis either in the child or the parents.

Lastly, the promptness and completeness of the restoration of the joints to normal may be noted. This is in harmony with other records, and supports and justifies the presentation of a favourable prognosis in these cases. It is a feature of the condition in pleasing contrast to the obstinate and often prolonged evil effects which so frequently attend the usual form of gonorrhœal arthritis. The explanation is doubtless to be found in the ease and readiness with which a purulent ophthalmia can be successfully treated as compared with the difficulty of bringing to a close the opportunities for absorption of a purulent discharge offered by the urethra and its related glandular recesses. The difference between the course of the arthritis in a case of gonorrhœa on the one hand, and in ophthalmia neonatorum on the other, lends emphasis to the statement that the checking of the urethral discharge is the first claim in a case of gonorrhœal arthritis. It can hardly be supposed that the delicate tissues of the child's joints are more resistant to the gonococcal poison than are those of the adolescent or adult, and the explanation of the course of the contrasted joint complications must therefore be found in the readiness with which the supply of poison is removed in the one case and the difficulty with which it is checked in the other.

ON THE CLINICAL SIGNIFICANCE OF DOUBLE OPTIC NEURITIS IN CHILDREN AND YOUNG ADULTS AND ON THE CAPACITY FOR RECOVERY FROM SYMPTOMS OF TUMOUR OF THE BRAIN.

THE cases which form the basis of this paper illustrate some of the circumstances in which double optic neuritis affecting children and young adults occurs in clinical practice. In none of them, fortunately, is the cause of the neuritis made absolutely certain by post mortem examination, but as they are records of clinical facts which demanded attention both from a diagnostic and prognostic point of view, they have a definite practical interest and value. An attempt has been made to increase this value by making the records as complete as possible, both by detailed examination of the patients, and by following the cases after the opportunity for continuous observation had passed away. In this respect it may be noted that whilst some of the cases were first seen when the symptoms, including the optic neuritis, were of recent development, others came under observation long after—in some cases years after—the acute conditions had subsided. Hence, assuming the two groups to include cases of one and the same order, the clinical picture they contribute to form is rendered fairly complete, the probable issue of the earlier cases being reasonably inferred from the known facts of those seen at a later stage. In any event, the records display some of the considerations and difficulties which arise in the practical handling of such cases, and at the same time contribute something to our knowledge of the clinical history of optic neuritis.

The patients are all young, their ages at the date of the first observation ranging from 10 to 21 years, but the date of the occurrence of the optic neuritis was not in any case later than the seventeenth year. Of the total of nine, six are females and three males.

The first question which inevitably arises on the recognition of a double optic neuritis is this—Is the neuritis due to a tumour of the brain? the word tumour being used, in the sense Dr. Hughlings Jackson defined it, as equivalent to a "foreign body" or "adventitious product" within the skull. An affirmative answer to this question is sometimes easy; at other times such a result can only be reached if all the evidence is carefully collected and weighed. On the other hand, a confident negative answer is almost always difficult, and for the most part is only justified when some other admitted cause for the optic neuritis can be certainly recognized; or when, in the absence of any such cause, and from the non-appearance of other evidence, the existence of a tumour is rendered highly improbable. The diagnostic position thus defined is the first practical issue from a study of the cases here under consideration.

Attention may now be directed to the evidence of cerebral disturbance actually present in the first seven cases—the evidence, in other words, which is taken as justifying the view that the optic neuritis was in all probability dependent in each case on the existence of an intracranial tumour.

These seven cases naturally fall into two groups—(1) those seen shortly after the development of the optic neuritis; (2) those in which the optic neuritis had long since passed into a condition of optic atrophy.

The first case (p. 95) presents evidences of cerebral disturbance in abundance. Obstinate vomiting, and pain in the head varying much in intensity, are sufficient of themselves, together with the optic neuritis, to make the diagnosis, but here we have, in addition, giddiness, retraction of the head, anosmia, and certain paralyses, not to speak of the mental phenomena and the slow pulse and subnormal temperature.

The second case offers a marked contrast to the abundant evidence provided by the first. With the exception of considerable pain in the head and optic neuritis there is no manifest evidence of cerebral disturbance, yet these will be allowed to be sufficient, and more especially as they receive confirmation from a study of the knee-jerks. The observations in this case were made under unusually fortunate circumstances, for, whilst the knee-jerks were present when the boy was first seen, they disappeared during his residence in hospital, and they have never since been detected. There is thus no doubt that the patient lost his knee-jerks during the time when he was the subject of headache and double optic neuritis almost certainly due to intracranial tumour. In two other of the cases (Nos. 5 and 7) here submitted with a diagnosis of intracranial tumour the knee-jerks were absent. No doubt, before such a conclusion in reference to the knee-jerk is justified, the examination demands great care, as unless this is exercised the jerk, though potentially present, may not be elicited. In examining these cases the precautions insisted on by Jendrassik, Buzzard, and Gowers have been borne in mind, and whenever possible the tests have been repeated and confirmed by other observers. The result is to show that of the seven cases the knee-jerks were absent in two (Cases 5 and 7), in one (Case 2) they disappeared under observation, and in another (Case 1) they varied at different stages of the case, being sometimes present or exaggerated and sometimes absent or diminished.

The point of importance just now is the restriction in Case 2 of the evidences of cerebral disturbance to double optic neuritis, headache, and loss of the knee-jerks, and the necessary inference arising therefrom, viz., that such loss may be a valuable confirmatory sign of intracranial disturbance. Yet it must be remembered that the loss of the knee-jerk did not occur until fully two months after the onset of symptoms; during the greater part of these two months the boy had no complaint to make except of pain in the head, and it was not considered necessary to seek medical advice until sight was failing. The earlier phases of the case were marked by nothing more than headache and optic neuritis. It is highly probable, therefore, that neglect to use the ophthalmoscope at that date would have led to a serious error of diagnosis.

In Case 3 a further illustration is afforded of the slightness of the evidence, apart from optic neuritis, in some cases of cerebral disease, presumably tumour. Though the patient had never been confined to bed even for a single day, yet symptoms of intracranial disturbance had undoubtedly existed for six or seven months. Every morning during this period the lad vomited and had some frontal pain, but at other times he appeared quite well. When medical aid is sought he is blind from optic atrophy, the result of neuritis which had doubtless existed for months. Here again, the first, and for long the only symptom, was vomiting, and this limited to one particular hour of the day. The vomiting and double optic neuritis were the only two events which occurred in a period of ten months, the vomiting being regarded by the parents as a trifling affair and only mentioned under some pressure. Yet it was the first, and probably for some time the sole evidence of an organic intracranial disease which has resulted in the permanent destruction of the patient's sight.

These three cases well display the cerebral events which may be associated with double optic neuritis as evidence of (presumably) intracranial tumour, and they show how widely such events may differ both in their number and severity.

The next four cases came under observation for defective sight dependent on optic nerve atrophy, this condition having been in existence for periods of time ranging from six months to ten years. In all but the last one the optic atrophy was manifestly the sequel to an optic neuritis, and the question may be raised whether this case (No. 7) ought to stand in line with the others. And all the more so, that whilst in the three other cases (Nos. 4, 5 and 6) there is a distinct history of a more or less severe illness with such symptoms as pain in the head and vomiting, the case now alluded to has no such history, but, on the contrary, the failure of sight is reported to have come on quite gradually and without any other symptom or evidence of any kind. There are, however, certain reasons which may be urged in support of the suggested diagnosis. In the first place, the absence of evidence of neuritis at the present date does not prove that neuritis never existed. "I am certain," writes Dr. Hughlings Jackson,[1] "that the most striking appearances of double optic neuritis . . . will pass away so far as to render it impossible for a careful observer seeing the patient for the first time, when recovered, to be able to declare that the peculiarities discoverable about the optic disc are pathological." Similarly, Sir Wm. Gowers[2] writes: "Great caution is necessary in inferring, from the appearance of discs long after the onset of the atrophy, that this was simple and not neuritic; . . . the characters of the latter may ultimately resemble very closely those of the former." The same point is emphasized by Dr. Buzzard.[3]

There is a further statement resting on the authority of Dr. Hughlings Jackson[4] which may be advanced in support of an arrangement that places Case 7 in line with the previous six. It is this: "There is sometimes an acute illness as our first evidence of tumour . . . but it is important to bear in mind that optic neuritis may occur without any headache and without vomiting, or that headache may be slight and the vomiting none." It must therefore be regarded as possible, that the condition of the optic discs in this patient is not due to a primary atrophy, but is subsequent to a pre-existent optic neuritis unaccompanied by any other sign of cerebral disturbance; and that the evidences of neuritis other than the atrophy of the discs have disappeared. But even if the atrophy of the optic nerves was primary in character, this would not exclude the diagnosis of cerebral tumour, though it is difficult to think of such a result except from a tumour situated so as to press on the optic chiasma,[1] whereas in the case now under discussion the presumption from the evidence is that the tumour is in the cerebellum. The point, however, need not be further laboured, as the evidence of intracranial disturbance is by no means limited to the condition of the optic discs. Further and sufficient proof is provided by the existence of nystagmus, by the absence of the knee-jerks, by the occurrence of "fits" of the *petit mal* order, and by outbreaks of temper which may be taken as evidence of depreciation of the cerebral tissues. Attacks of a similar character are noted in Case 6, where the diagnosis of tumour can scarcely be questioned (see also Case 2).

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[1] *Medical Times and Gazette*, 1873, Vol. ii, p. 541.

[2] "Medical Ophthalmoscopy." Third Edition, p. 166.

[3] "Diseases of the Nervous System." p. 148.

[4] *Transactions of the Ophthalmological Society*, 1880-81, Vol. i, p. 72.

Taken together, the four cases of the second group confirm and extend the lesson enforced by the members of the first group. They show that the degrees of cerebral disturbance which accompany optic neuritis of intracranial origin may vary widely, and they further demonstrate how largely the results of such disturbance may pass away, although the injury to the visual apparatus may be both permanent and pronounced.

It is possible that the question may be raised whether after all these cases are really cases of intracranial tumour, and especially in view of the evidences of recovery which they exhibit. And it may be admitted that, reasoning on *a priori* grounds, or considering only the numerous undoubted cases of tumour in which the patients suffer severely, and which pass with comparative rapidity to a fatal termination, there is some difficulty in accepting the diagnosis here suggested. But it cannot be denied that in all the cases there is evidence of definite organic mischief within the skull, and the facts of experience do not seem to offer any choice other than tumour or meningitis. Taking the mere occurrence of recovery into consideration, it is certain that the diagnosis of meningitis, at least of tubercular meningitis, is not less difficult than that of tumour. Then in three of the cases the patients never had in the general sense of the term any illness which could reasonably be regarded as an attack of meningitis, and in those who were at any time seriously ill there were in two cases signs of a localized lesion in the brain, such as paralysis of a limb or a hemiplegic attack. Possibly, though certainly very rarely, such paralyses may occur in meningitis, but the presumption from these incidents is decidedly in favour of tumour. Again, it is to be remembered that in several of the patients the evidence of active intracranial disease extended over many weeks, and even months—a state of matters difficult to reconcile with a diagnosis of meningitis. And if the cases were cases of tubercular, and presumably therefore of basal, meningitis, the absence of disturbance of the functions of the ocular muscles is certainly remarkable. Further, in the one patient seen throughout the whole course of her very serious illness, the temperature, so far as it was disturbed, had a range below the normal level, and two other members of the series seen with more or less acute symptoms were also free from evidence of fever. (Cases 1, 2 and 3)

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[1] Handford, *Brain*, 1892, Vol. xv, p. 463; also Hinshelwood, *Glasgow Hospital Reports*, Vol. i, p. 4.

The degree of the optic neuritis is also of importance in the discussion of this question. Optic neuritis is undoubtedly present in a certain number of cases of meningitis, but it is almost invariably of slight, or, at the outside, of moderate development. But in all the present seven cases—with the possible exception of the last—the degree of optic neuritis either was, or had been, of the considerable or extreme degree usually found with, and indeed strongly suggestive of, intracranial tumour. The balance of probability, even when attention is confined to the facts of the present cases, thus strongly inclines to the diagnosis of tumour rather than to that of meningitis. Other considerations support this view. Thus it is a fact that a tumour, or even tumours, may develop in the brain and yet fail to give the slightest clinical evidence of their existence. Such growths, too, may be of considerable size, and may occupy parts of the brain which, to judge from general experience, are by no means tolerant of disturbance. The cerebellum, the frontal lobe, the occipital lobe, the temporo-sphenoidal lobe, even the motor tract, have each in turn been found to be the seat of a tumour which during life gave no evidence of its presence.[1] A step in advance of these cases is provided by a further series in which, with a tumour of the brain (proved by post mortem examination), there has existed some, but only slight, evidence of cerebral disturbance, the evidence being, in some of these cases, equivocal or non-suggestive of tumour.[2] A further contribution to the argument is found in records of cases in which severe symptoms of brain tumour have subsided, to be followed after an interval, and sometimes after a long interval, by return of the symptoms and death of the patient, the diagnosis of tumour being confirmed by post mortem examination.[3] Indeed, the recorded instances of verified cases of brain tumour form a series in which, commencing from those absolutely without symptoms, there may be traced all degrees of severity in the number and degree of the disturbances produced.

In reference to the question of recovery, Dr. Hughlings Jackson[4] writes that "it is not at all uncommon for a patient to get rid of all symptoms of tumour except that he remains blind or defective in sight," and in discussing the diagnosis as between meningitis and tumour he adds: "But when recovery follows, what are we to say? . . . I still think them to be cases of tumour." Dr. T. K. Monro[5] has recorded a case which comes as near actual demonstration as possible of the recovery of a patient from the effects of a brain tumour, with the exception of optic atrophy and blindness. The patient, a man of 63 years, died in the Glasgow Royal Infirmary from cancer of the stomach. He was the subject of optic atrophy, and was almost blind. For thirty-three years he had been an inmate of the Blind Asylum, the loss of sight being attributed to a severe illness which confined him to bed for almost twelve months when he was 16 years of age. The knee-jerks were normal. The post mortem examination showed a myxomatous tumour almost replacing the left half of the cerebellum, with atrophy of the optic chiasma and nerves. Such a record materially strengthens the decision for a diagnosis of tumour in such cases as are here under consideration.

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[1] See Table I, p. 91.

[2] See Table I, p. 91.

[3] See Table II, p. 92.

[4] *Medical Times and Gazette*, 1873, ii, p. 197.

[5] *Glasgow Medical Journal*, 1896, ii, p. 173.

Thus the facts of the present cases, together with the presumption arising from a study of other cases, form a strong foundation on which the diagnosis of intracranial tumour seems securely placed. It would certainly be difficult to find anything like so strong a foundation for a diagnosis of meningitis.

In reference to the disappearance of symptoms that have become fully established, it has to be borne in mind that the usual clinical effects of an intracranial tumour are due not so much to the pressure or mechanical displacement which the tumour produces, as to the vascular and inflammatory disturbances which are excited in its neighbourhood. Given cessation of growth of the tumour, it is to be expected that such disturbances will subside, with a corresponding diminution in the symptoms due to them.

Assuming, then, that these seven cases are examples of the clinical exhibition of the effects of intracranial tumour, two conclusions follow. First, as already observed, the physician must be prepared to expect in different cases very different amounts of evidence—sometimes of such a nature and degree as to proclaim loudly the existence of intracranial disease, at other times slight in amount, and not manifestly cerebral in origin. It is in these latter cases especially that the ophthalmoscope renders such useful service, for whilst optic neuritis is not present in every case of brain tumour, it exists at some stage or other in the great majority, and not infrequently it is an early, if not the earliest evidence. Even when existing alone, and more especially when it affects both eyes and is distinguished by much swelling of the papilla, it demands the consideration of cerebral tumour as a possible diagnosis, though, as will be shown later on, it may have exactly the characters just described and yet be due to some other and much less serious cause than tumour of the brain.

The second conclusion which follows from the acceptance of intracranial tumour as a diagnosis in the seven cases now under discussion, is that the prognosis of such cases so far as life is concerned is by no means hopeless. One patient (Case 7) has survived the loss of sight by ten years and enjoys good health except for an occasional quasi-epileptic attack, another (Case 4) has a similar experience extending over four years, a third for three years, and the rest, though less impressive, are similar in the direction of their testimony.

Several series parallel to the seven cases here submitted are on record. So far back as 1866 Mr. (now Sir) Jonathan Hutchinson[1] tabulated a series of twelve cases of optic neuritis in children and following a more or less severe illness marked by delirium and other head symptoms, and he was able to show by the subsequent histories of the cases that frequently such children regain good health excepting as regards sight. The same author[2] also reported a number of similar cases in 1879. The precise nature of the primary lesion he considered doubtful, but was disposed to distribute the cases among tubercular meningitis, basal arachnitis, cerebro-spinal meningitis, and some of the exanthemata, as the probable causal conditions. Dr. T. K. Monro[3] has published three cases of blindness following double optic neuritis, other than the one already quoted, and regards each case as one of intracranial tumour. Indeed it seems difficult to avoid this conclusion in reference both to his and other similar cases unless the accepted significance of the combination of pain in the head, vomiting, and double optic neuritis is to be seriously challenged.

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[1] *Royal London Ophthalmic Hospital Reports*, 1866, Vol. v, p. 307.

[2] *Ibid.*, 1879, Vol. ix, p. 124.

[3] *Glasgow Medical Journal*, 1897, ii, p. 367.

The possibility of recovery from symptoms, and even severe symptoms, of brain tumour must necessarily be considered in determining the line of treatment to be pursued. The recognition of such a possibility cannot fail to infuse not only a more hopeful tone into the prognosis but also increased resolution into the treatment. Probably most will agree that when the diagnosis of intracranial tumour is accompanied by localizing symptoms, surgical interference should be anxiously considered; that in any case where this is not deemed advisable, medical treatment must consist of attempts to deal with the more troublesome symptoms; and that in the event of recovery occurring the measures indicated are those calculated to promote and secure the general vigour of the patient. Recovery, however, is a matter of degree, and at least two (1 and 5) of the present cases illustrate the dangers which may attend the patient even long after the original illness has subsided—a fact important to the prognosis, and also significant in reference to the anxious care with which these patients should be guarded. Dr. Hughlings Jackson[1] is especially emphatic upon the great necessity for caution in framing the prognosis in any case presenting symptoms of intracranial tumour. He points out, not only the liability to return of the symptoms after a period of quiescence, but also the very extended duration of the symptoms in occasional cases. Another danger on which he insists is that of sudden death, which may occur even when the patient appears to be doing well. The extent of this last risk receives striking illustration in a careful and elaborate analysis of 100 cases of brain tumour published by Dr. Hale White.[2]

There is another point to consider in reference to treatment. All the patients referred to in this paper as examples of recovery from the symptoms of intracranial tumour have this very serious qualification of their recovery, namely, that each one is blind. Recognizing that this is a very likely issue, that it may indeed be a relatively good result of the case, the best perhaps to be hoped for, it is of great moment to consider whether any means can be adopted to secure the patient against so grave a defect. Dr. Hughlings Jackson had considerable confidence in large doses of potassium iodide with or without mercurial inunction. There seems no doubt at all of the fact that trephining the skull, altogether apart from the removal of a tumour, is sufficient, at least in some cases, to cause the subsidence of the optic neuritis. This measure has been advocated both by Sir Wm. Macewen and Sir Victor Horsley;[1] and Dr. Jas. Taylor[2] has also called attention to it. The question is one of great importance, for, from such cases as are recorded here, and from the statement that the neuritis will subside under trephining, there would seem ground for hoping that in a certain number of cases of intracranial tumour where the situation of the growth cannot be localized, operative means may still assist in promoting the patient's comfort, and, given a natural tendency for the growth of the tumour to cease, may assist him to many years of good general health and unimpaired vision. One difficulty in urging on the patient what he at least will consider heroic measures is that occasionally all the evidences of intracranial tumour, including optic neuritis, subside spontaneously.[3] Another arises from the fact that optic neuritis may occur apart from intracranial tumour, and whilst at the time of its development and existence it may be impossible to say tumour is not the cause—for undoubtedly optic neuritis may be for long the sole evidence of tumour—the subsequent history of the case renders such a diagnosis in the highest degree improbable.

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[1] *Op. cit.*, p. 195 *et seq.*

[2] *Guy's Hospital Reports*, 1885, p. 117

This latter difficulty is illustrated by Cases 8 and 9 here recorded. In Case 8 (p. 107) not only was there double optic neuritis, but there was also paralysis of the right and possibly slight impairment of the function of the left sixth nerve, an event in the circumstances as significant of cerebral disturbance as well could be. Was such a combination indicative of tumour? In the early stage of the case it seemed impossible to deny that tumour might be the explanation, but the subsequent history renders such a diagnosis very improbable. There were no cerebral symptoms other than the optic neuritis and the ocular paralysis, but it was difficult to conceive of any explanation of these apart from intracranial mischief. No doubt the girl was anæmic, and anæmia is said to be one of the causes of optic neuritis. But anæmia, unless indirectly through the agency of a thrombosis or hæmorrhage, will scarcely produce a sudden diplopia and complete paralysis of a cranial nerve; and optic neuritis is itself a very rare and unusual circumstance in anæmic patients. An examination of every patient admitted to Sir Wm. Gairdner's wards for a period of five years did not reveal a single instance of the occurrence of optic neuritis with simple anæmia or chlorosis. Considering how very common cases of chlorosis are, how not only relatively but absolutely rare it is to find optic neuritis accompanying this condition, it becomes a question whether, when such association does occur, some new factor has not complicated the problem. The possible gravity of the significance of double optic neuritis certainly renders it most inadvisable to consider the mere existence of anæmia an adequate explanation of its presence, at least until all other known causes have been excluded, and even then, as already remarked, the extreme rarity of the association of the two conditions justifies some suspicion of the accuracy of their suggested relationship.

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[1] *British Medical Journal*, 1893, ii, p. 1365.

[2] *Transactions of the Ophthalmological Society*, 1884, Vol. xiv, p. 105.

[3] See Table II, p. 92.

The case (No. 8) is an example of double optic neuritis existing with a circumstance strongly suggestive of intracranial mischief (namely, paralysis of the sixth nerve), yet almost certainly the case is not one of tumour. The difficulty of providing a satisfactory diagnosis is great. A very similar case is recorded by Sir Wm. Gowers,[1] but the patient, in addition to optic neuritis and sixth nerve paralysis, suffered from attacks of severe occipital pain and vomiting, and the view taken of all these symptoms was that they were due to a transient localized meningitis. Possibly some help in understanding the present case (No. 8) is afforded by some observations recorded by Bristowe[1] noting the development of optic neuritis in anæmic patients, with evidence of thrombosis of some of the cerebral sinuses and veins. One patient, a girl of 19 years, when under treatment for anæmia, had a severe pain, first in the neighbourhood of the right ear, and later, a similar experience on the left side, with, on each side, tenderness and swelling suggesting thrombosis of the internal jugular vein. She was found to have double optic neuritis, and vomiting and headache were troublesome. After a few days she improved, but convalescence was interrupted by an attack of phlebitis in the leg. Bristowe's suggestion is that the attacks of pain about the ears meant thrombosis in the lateral sinuses, and he supports this by the record of a second case. This patient, a woman, aged 23 years (six weeks pregnant), suffered for twenty-five days from headache, sickness, pain and tenderness over the left mastoid, and general drowsiness; during her illness she developed a right hemiplegia, which had nearly passed off, when she suddenly died, apparently from syncope. There was double optic neuritis. Post mortem examination revealed thrombosis of the left lateral sinus, of the internal jugular vein, and of some of the veins on the surface of the brain. No evidence of disease in the ear, bones of the skull, or meninges could be found, and there was nothing to suggest any cause for the thrombosis other than the anæmia. These are certainly very important and interesting cases, and they afford a possible explanation of the appearance of optic neuritis in some cases of anæmia. In the present case (No. 8), too, it is not difficult to imagine a thrombosis or resultant hæmorrhage so situated as to interfere with the function of the sixth cranial nerve.

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[1] *Transactions of the Ophthalmological Society*, 1880-81, Vol. i, p. 115.

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[1] "Clinical Lectures and Essays on Diseases of the Nervous System." p. 184.

The occurrence of double optic neuritis in young women has been attributed to menstrual and uterine derangements, but the pathology of the combination is quite obscure. In connection with such cases, Dr. Bristowe's experience should not be forgotten. In a case reported by Broadbent,[1] in which headache, vomiting, and double optic neuritis had developed on sudden arrest of the menses, death ultimately occurred, and the post mortem examination showed neither tumour nor meningitis—"the only morbid appearance in the brain was effusion into the ventricles." A second patient, apparently of the same order, lost all her symptoms on re-establishment of the menstrual flow, but was blind from optic atrophy.

The eighth case of the present series is certainly not a riddle easily read, but it is at least a demonstration that double optic neuritis, even when combined with an event so suggestive of intracranial disease as paralysis of a cranial nerve, does not necessarily mean a diagnosis of tumour, and that such a combination is not inconsistent with a rapid and practically a complete recovery. But how is such a case to be distinguished at the outset from cases such as the earlier members of the series here submitted? For, if double optic neuritis and one such symptom as headache (Case 2), or vomiting (Case 3), is sufficient to justify a diagnosis of cerebral tumour, or, at least, to demand a very anxious prognosis, the same must be true of double optic neuritis when associated with such an event as a sixth nerve paralysis. The fact seems to be that there is no absolutely sure ground on which to base a distinction other than that provided by an exact knowledge of the progress of the case. When there is double optic neuritis there is ground for anxiety; whether it means definite organic disease within the skull or not is in many cases difficult to say. But it is certain that even when there are other suggestions of cerebral disturbance the issue may be most satisfactory, whilst, on the other hand, the neuritis may stand almost alone as a sign of such disturbance, and yet the patient's eyesight, or even his life, be in great peril. In individual cases there may be particular features which will help the distinction between the two classes thus defined, but, on the whole, it is from a study of the development and progress of the case that the most reliable guidance will be obtained. How different, for example, is the character of the movement in Case 8 from what occurred in the earlier members of the series. It is not the mere existence of optic neuritis, of headache, of vomiting, etc., that gives cause for alarm, so much as the persistence, aggravation, or expansion of any one, or combination, of these symptoms. Further, the knowledge of the progress of the case on which an estimate and outlook are to be based must not be too limited in its range, for there can be no doubt at all that the urgency of the symptoms in many cases of cerebral tumour varies from day to day in the most remarkable manner. In Annie K. (Case 1, p. 95) this was repeatedly seen; every now and then during the time when a fatal termination seemed probable, a day or two would occur in which pain and vomiting would entirely cease. "You go to a patient one day," writes Dr. Hughlings Jackson,[1] "and find him curled up in bed suffering intense pain, and hear that he has been vomiting urgently. Next time you go to see him you find him sitting by the fire reading the newspaper, apparently quite well. *Do not be deceived by these pseudo-recoveries.*" It is a steadily continued, rather than a sudden, diminution of the symptoms which justifies a lessening of the anxiety necessarily born of the discovery of double optic neuritis. This is well seen in Case 9 (p. 109), where there existed double optic neuritis with such a large amount of swelling that, taking the ophthalmoscopic examination alone, the diagnosis of tumour seemed almost inevitable. No doubt the complete absence of all other cerebral symptoms was a grateful sign, and the history—if it could be relied on—of very rapid loss of sight made one think, or rather hope, that some other interpretation was possible. But it was only the steady movement of the case in the right direction that lessened the anxiety of the prognosis and justified an expectation of still better things. What exactly the diagnosis here ought to be may possibly be a question, but the case emphasizes what has just been stated in regard to the questions of diagnosis and prognosis arising in connection with attacks of double optic neuritis. The diagnosis now suggested is that of retro-bulbar neuritis. No doubt such a condition usually affects one eye only, or occasionally one eye, and then after an interval the other. There seems no reason why it should not attack both eyes at one and the same time. Again, the diagnosis of retro-bulbar neuritis usually includes no appreciable early ophthalmoscopic signs, or only evidences of slight neuritis. But the evidences of papillitis certainly vary widely, and here at the worst it is a matter of degree. The rapid and early loss of sight, the history of pain in the eyeballs, the condition of the pupil light-reaction, the fact of nearly complete recovery, and the restoration of the periphery of the visual fields before central vision was possible, are all in favour of a retro-bulbar lesion. That both nerves were affected at one and the same time, and that there was also considerable swelling of the optic discs, are interesting features which give individuality to the case. The rapid failure of vision in this case may be usefully compared with the gradual decline of sight in the cases regarded as dependent on organic intracranial disease—probably tumour. In these latter cases it is not as a rule during the stage of swelling and exudation that sight is prejudiced, but rather when the inflammatory movement is subsiding and the newly formed fibrous tissue, gradually becoming more compact, is, by the resulting pressure, injuring the vitality of the nerve elements.

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[1] *Transactions of the Ophthalmological Society*, 1880-81, Vol. i.

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[1] *Medical Times and Gazette*, 1873, Vol. ii, p. 541.

It is of course a matter of common knowledge that double optic neuritis, as in Case 8, may exist without any complaint of defective sight and without any recognized failure when the usual tests for the visual acuity are applied. This was taught by Dr. Hughlings Jackson more than thirty years ago. The present cases show three possible conditions of the visual acuity with double optic neuritis. First, vision may be practically unaffected during the whole course of the neuritis (Case 8); it may be almost or completely lost during the stage of exudation and yet may subsequently be entirely regained (Case 9); or, as in the majority of cases, though it may be little or not at all affected during the stage of acute exudation, it may in the end be entirely destroyed (post-neuritic atrophy).

The diagnosis in all these nine cases may possibly be questioned. As has been already suggested, it may be asked whether any of the first seven is really a case of tumour of the brain. On the other hand, it may be urged that Cases 8 and 9 ought, equally with the earlier members of the series, to be placed under this diagnosis, and that they must be regarded as differing from the earlier cases only in presenting slighter clinical evidence and a more complete recovery. It may be impossible to maintain an absolutely confident negative to this latter proposition, just as it is impossible—in view of the fact that occasionally a tumour of the brain is found when no evidence of its presence existed during life—to be quite certain that any particular individual, though apparently in perfect health, may not be the subject of some intracranial growth. One must accept what seem to be the reasonable probabilities of the case. Thus, when in children and young adults (as in Cases 8 and 9) the evidence of intracranial disturbance other than optic neuritis is little or none, when there is relatively rapid subsidence of the neuritis and of any other existing symptom, and when, also, during such evidences of improvement no fresh symptoms emerge, it is justifiable to regard the optic neuritis as probably non-indicative of tumour, and to speak of the prospects of the patient in fairly confident and hopeful terms. But when the conditions attending double optic neuritis are the opposite of those just stated, the existence of gross organic disease is highly probable, if not absolutely certain, and the risks to life, or alternately to sight, are considerable. Some assistance to prognosis, if not also to diagnosis, is gained by treatment, and if under complete rest, mercurial inunction, and potassium iodide—or in the case of anæmic girls, iron—internally, the symptoms, whatever they be, do not show with a fair degree of promptitude decided improvement, the probability of organic disease is great, and the prognosis is correspondingly anxious. The differential diagnosis of tumour and meningitis must then be discussed, and in the event of the former being accepted the question of surgical interference at once arises. Unless there are evidences of the exact locality of the tumour, operation with a view to its removal could hardly be proposed, and statistical evidence shows very decidedly that in the great majority of cases of intracranial tumour removal is out of the question. Further, any proposal for an operation must be considered in the light of the fact that in a certain number of cases patients make a good recovery, with or without impairment of sight, under medicinal treatment, or, indeed, with no treatment at all; it must also be recollected that there may be more than one tumour, a state of matters not uncommon in young patients.

The practical summing up seems to be that when optic neuritis, with or without other symptoms, is present in children or young adults, such medicinal measures as have already been indicated should be energetically applied. Should these fail, and should there exist localizing symptoms as, *e.g.*, attacks of Jacksonian epilepsy, the possibility of surgical removal of the tumour comes seriously into view. In the absence of localizing evidence, and on the failure of medicinal measures, it is a fair proposal to trephine the skull with a view to relieve symptoms. Undoubtedly headache and optic neuritis may be relieved in this way, and the patient may thus be rendered more comfortable even if his life is not prolonged. If sight is to be benefited by trephining, the operation must not be too long delayed; it must be undertaken during the exudative stage and before the nerve fibres have been structurally damaged by the pressure of the newly organized fibrous tissue.

There is one other practical point which may be noted. It arises from the possibility of optic neuritis being associated with, or perhaps due to, menstrual or uterine disturbances. Such cases as those reported by Broadbent[1] demonstrate this possibility, and it is supported by various authorities.[2] Hence, when the patient is a female, and especially if there is absence of normal menstruation, attention to this function ought to be included in the treatment. The coincidence of suppression of the menses with optic neuritis must not, however, lead too confidently to the conclusion that the one is the cause of the other, and to a correspondingly cheerful prognosis. In Case 1, in which the clinical evidence of tumour is complete, the patient ceased to menstruate at a very early stage of her illness, and the function for a long period remained in abeyance. Presumably the general functional disturbance resulting from the tumour interfered with menstrual activity just as it did with other of the bodily functions. Whilst, therefore, it may be said that there is evidence to support the view that optic neuritis may in some way or other result from menstrual irregularity, it must also be allowed that a tumour of the brain may in some indirect manner lead to suppression of the menses.

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[1] *Transactions of the Ophthalmological Society*, 1880-81, Vol. i, p. 108.

[2] *Ibid.*, 1880-81, Vol. i, p. 61.

TABLE I.

CASES OF TUMOUR OF THE BRAIN WITHOUT SYMPTOMS OR WITH SYMPTOMS OF DOUBTFUL SIGNIFICANCE.

Nature and Position of Tumour	Author
Cancerous tumour of greater part of anterior lobe, secondary to cancer of the lung. Nothing cerebral observed except manner at times peculiar and replies to questions "odd."	Hughlings Jackson, *Medical Times and Gazette*, 1873, Vol. ii, p. 195.
Tumour of each lobe of cerebellum. No disturbance of gait or other motor symptoms.	Hughlings Jackson, *Ibid.*
Psammo-sarcoma, measuring 2 by 1½ inches, growing from falx at junction with tentorium and compressing corpus callosum.	Goodhart, *Pathological Society's Transactions*, 1885-1886, Vol. xxxvii, p. 16.
Mixed-cell sarcoma, of considerable size, in ascending parietal and supra-marginal convolutions; also a smaller tumour in occipital lobe.	D'Arcy Power, *Ibid.*, p. 54.
Myxo-chondroma, measuring 1½ inches in longest diameter, springing from the falx and loosely embedded in a depression on the median aspect of ascending parietal convolution.	Hadden, *Ibid.*, p. 71.
Carcinoma, 1½ inches in diameter, affecting dura mater and skull over right orbit. A prominence was observed over the right orbit but no symptoms of cerebral disease.	F. Taylor, *Ibid.*, p. 72.
Psammoma (two), each ½ inch diameter, springing from choroid plexus of each lateral ventricle and occupying symmetrical portions of each descending cornu.	Hadden, *Ibid.*, p. 74.
Tumour, size of Tangerine orange, in occipital and temporo-sphenoidal lobes.	Hale White, *British Medical Journal*, 1886, Vol. i, p. 117.
Tubercular mass in cerebellum.	Hale White, *Ibid.*
Tumour in frontal lobe. Only symptom, headache, until shortly before death, when optic neuritis appeared. Death sudden.	Hale White, *Ibid.*
Extensive tumour of orbital convolutions, destroying one olfactory nerve and producing considerable depression in bone. No cerebral symptoms unless "shortness of temper."	Alexander, *Liverpool Medico-Chirurgical Journal*, 1888, p. 253.
Large spindle-celled sarcoma in substance of hemisphere. Patient had occasional fits for ten years—controlled by potassium bromide—but no optic neuritis or other evidence of tumour.	Hadden, *Brain*, 1888-1889, p. 523.
Considerable cystic tumour in anterior part of temporo-sphenoidal lobe. Patient for several years under observation for "epileptic mania." No other evidence of tumour.	Trowbridge, *Journal of Nervous and Mental Diseases*, 1891, p. 217.
Large tumour of pre-frontal lobe. Optic neuritis for a long time, no other symptom until thirty hours before death.	Shweinitz, *Journal of American Medical Association*, 1893, Vol. xxi, p. 607.
Numerous tubercular tumours both in sensory and motor regions. Patient, a child, quite free from head symptoms until a day or two before death, when some evidences of meningitis.	Middleton, *Lancet*, 1893, Vol. ii, p. 137.

TABLE II.

CASES OF RECOVERY, MORE OR LESS COMPLETE, FROM SYMPTOMS OF BRAIN TUMOUR.

Symptoms	Sequel	Author	Notes
Girl, 11 years, headache, vomiting, optic neuritis.	Recovery complete except blindness.	Hughlings Jackson, *Medical Times and Gazette*, 1865, Vol. i, p. 626.	Patient unable to stand, but could walk well with very slight guidance.
Woman, 19 years, headache, paresis of abducent and facial nerves on each side, with double optic neuritis.	Four months after, free from pain, and generally well, but some degree of strabismus and facial paresis.	Wilks, *British Medical Journal*, 1870, Vol. ii, p. 61.	The diagnosis of tumour suggested, but with some hesitation.
Man, 23 years, intense headache, convulsive seizures, optic neuritis.	Recovery to extent of walking about ward and reading newspaper, then sudden death (hæmorrhage from gliomatous tumour).	Hughlings Jackson, *Medical Times and Gazette*, 1873, Vol. ii, p. 195.	Illustrates risk that attends even "recovery" from cerebral symptoms (see page 81).
Woman, 59 years, severe paretic and mental symptoms followed by headache, vomiting, and optic neuritis.	Illness extending over 5 months interrupted by periods of marked improvement—one of these lasting for 3 weeks. Death : glioma in white substance of posterior lobe.	J. B. Yeo, *Brain*, 1878, p. 273.	
Woman, 23 years, presumptive signs of tumour of cerebellum.	A year later general improvement, also some improvement in vision.	Gowers, *Transactions of Ophthalmological Society*, 1881-82, Vol. ii, p. 34.	Paresis of upward movement of eyes was a special feature. Tendon-jerks exaggerated.
Woman, 19 years, extreme double optic neuritis with perfect vision for five months.	Then rapid and complete failure of sight, with optic atrophy—no other symptoms.	West, *Ibid.* 1882-83, Vol. iii, p. 136.	Patient had been subject to chlorosis.
Girl with headache and optic neuritis.	Recovery under administration of iron.	Hale White, *British Medical Journal*, Vol. i, p. 117.	Stated to be a case of anæmia.
Woman, 17 years, diplopia, strabismus, optic neuritis, weakness of lower limbs, absence of knee-jerks.	At end of six months quite well, vision normal. Recurrence of symptoms after 18 months, but again recovery.	James Anderson, *Ophthalmic Review*, 1886, Vol. v, p. 121.	

TABLE II.—*Continued.*

Symptoms	Sequel	Author	Notes
Woman, 18 years, headache, vomiting, tinnitus, optic neuritis. Patient anæmic; no syphilis.	Sodium iodide and mercurial inunction ordered. Patient completely well in two months. Six months later reported, "General health excellent, vision perfect."	James Anderson, *Ibid.*, p. 126.	
Man, 36 years, headache, hemiplegia, delirium, coma, optic neuritis.	Recovery complete under mercuric chloride and potassium iodide.	Suckling, *Birmingham Medical Review*, 1889, p. 159.	Recognized by author as probably a case of gumma, though no history or evidence of syphilis.
Woman with headache, vomiting, marked optic neuritis. No history or evidence of syphilis.	Recovery complete under use of potassium iodide.	James Taylor, *Lancet*, 1894, Vol. i, p. 133.	Author expresses his view that success of treatment is no proof of syphilitic causation.
Young man with headache, vomiting, intense optic neuritis, and epileptiform attacks.	Recovery apparently complete; two years later reported in perfect health.	Ransom, *Brain*, 1895, p. 534.	Renal disease, syphilis, plumbism, could all be excluded.
Woman, 39 years, headache, diminished mental power, convulsions, optic neuritis; no evidence or history of syphilis.	All symptoms disappeared under mercury and potassium iodide. For many years health good, though occasional return of symptoms in a slight form and always yielding to treatment.	Althaus, *Transactions of the Clinical Society*, Vol. xxxix, 1896, p. 39.	
Boy, 7 years, headache, vomiting, optic neuritis, paresis of left oculo-motor nerve, knee-jerks absent.	Disappearance of all the symptoms except blindness from optic atrophy. Fifteen months later general health reported to be good.	T. K. Monro, *Glasgow Medical Journal*, 1896, Vol. ii, p. 173.	
Man, 63 years, history of severe head symptoms at 16 and followed by blindness; knee-jerks normal.	Good health until shortly before death from cancer of stomach. P.M. = myxomatous tumour in cerebellum.	T. K. Monro, *Ibid.*, p. 176.	

TABLE II.—*Continued.*

Symptoms	Sequel	Author	Notes
Woman, 16 years, serious illness of three to four weeks' duration, severe headache, vomiting, optic neuritis, and ocular paresis. Temperature subnormal, pulse slow, knee-jerks almost abolished.	Recovery complete except for very serious defect of vision (optic atrophy).	Alexander Napier, *Ibid.*, p. 372.	Author regards the case as one of tumour complicated by a transient attack of meningitis.
Girl, 8 years, serious illness, with headache, vomiting, constipation, and loss of vision.	When seen at 17, well, except for blindness and slight paresis on one side—occasional epileptic seizures since puberty.	T. K. Monro, *Ibid.*, 1897, Vol. ii, p. 367.	
Woman, 17 years, some months of headache followed by vomiting and double ptosis; optic neuritis on examination and diminished knee-jerks.	Improvement for some weeks, then relapse, followed by almost complete recovery. Vision said to be "good" but evidence of post-neuritic atrophy.	T. K. Monro, *Ibid.*, p. 369.	

Sir Jonathan Hutchinson's cases of double optic neuritis in children associated with severe illness and various head symptoms, and followed by recovery except for partial or complete blindness, are recorded in the *Royal London Ophthalmic Hospital Reports*, Vol. v (1866), p. 307, and Vol. ix (1879), p. 124. In the same publication, Vol. vi (1869), p. 43, Sir Jonathan reports the case of a patient who suffered from pain in the head, ptosis, convulsions, and sudden blindness, and who in the course of six weeks, though still blind, was otherwise quite well. The same author [*Reports*, Vol. xii (1889), p. 65] has described two cases of double optic neuritis occurring without explanation in young men; one of the patients was seen eleven years later when he was the subject of double optic atrophy, but his general health was quite undisturbed.

Whatever may be the explanation of these cases, they manifestly have very close clinical relations with those recorded with this essay. The earlier ones afford abundant confirmation of the statement that double optic neuritis, even when associated with severe cerebral disturbance, by no means demands anything like a hopeless prognosis as regards the life of the patient, though the risk of permanent damage to vision is great. The last two cases further show that this danger to vision may also exist even when no other cerebral events accompany the optic neuritis; in the parallel case (No. 9, p. 109) of the present series the patient, however, regained practically perfect vision.

Further cases illustrative of a capacity for recovery from severe symptoms of cerebral disease will be found in the late Dr. Bristowe's "Clinical Lectures on Diseases of the Nervous System." And, of course, on all points concerning the diagnosis and prognosis in cases of intracranial tumour invaluable assistance is to be gained from the several papers of Dr. Hughlings Jackson. See *e.g.*, *Royal London Ophthalmic Hospital Reports*, 1863-65, Vol. iv, p. 389 *et seq.*; *Medical Times and Gazette*, 1873, Vol. ii, p. 139 *et seq.*; *Transactions of the Ophthalmological Society*, 1880-81, Vol. i, p. 60; and *British Medical Journal*, 1888, Vol. ii, p. 59.

CLINICAL RECORDS ILLUSTRATING RECOVERY FROM SYMPTOMS OF BRAIN TUMOUR.

CASE 1.—*Severe illness characterized by symptoms of cerebellar tumour and followed by recovery except for total loss of sight (optic atrophy). Return of symptoms after two years' interval of good health, with subsequent complete convalescence. Patient reported free from all symptoms, except blindness, six years after first illness.*

Annie K., aged 16, admitted to Glasgow Western Infirmary July 28, 1893, complaining of vomiting, pain in the forehead, and unsteadiness of gait.

She enjoyed good health until two months before admission, when vomiting began, at first only on rising in the morning, but afterwards also throughout the day, both immediately after food and in the intervals between meals. In a week or so frontal pain became troublesome; the degree of it has varied much, but it is always present in a greater or less measure. She has been told that her eyes have recently become more prominent, the amount of protrusion varying from time to time. Her sight has been so bad that on several occasions she has run against objects in the house and street. Still, three weeks ago she went out to domestic service, though she soon found herself unfit for this, chiefly, it would seem, on account of her defective sight. Shortly after returning home she began at times to be very giddy; to stagger, with an inclination to fall backwards, when walking; and more recently she has complained of pain in the right side of the neck. Menstruation, which previously had been regular, had ceased during the last two months.

Patient is a well-nourished, healthy-looking girl. She is somewhat lethargic, and is slow in answering questions, but quite intelligent. There is marked retraction of the head. When assisted to rise she becomes giddy, and tends to fall backwards; indeed, she would fall unless supported.

The pupils are equal and contract fairly well both to light and in convergence. In each fundus there is marked papillitis; visual defect is considerable; no squint or nystagmus; possibly slight exophthalmos on each side. No tenderness on percussion of the skull. The superficial reflexes are normal; each knee-jerk is very marked—almost certainly exaggerated; cutaneous sensibility seems everywhere normal, and there is no paralysis of the limbs.

Thoracic and abdominal viscera normal to physical examination, and urine normal except for some phosphatic excess.

The sense of smell is very defective; she has observed this herself. Hearing not definitely abnormal.

The condition of patient during her first month of residence varied very much. Occasionally she would seem fairly comfortable, but for the most part frontal pain was extreme and vomiting very obstinate. At times she passed into a semi-comatose condition and appeared very likely to die. The bowels could not be evacuated without enemata, and unless the catheter was regularly used urine was passed in bed. Temperature almost invariably subnormal; pulse from 60 to 72. The swelling of each optic papilla became less, the light-response of the pupils diminished, and the vision was reduced to mere perception of light. Tested on several occasions, the knee-jerks were found to be absent.

On August 23 some evidence of right facial paresis was observed, the upper as well as the lower muscles being involved; general condition somewhat improved.

On September 2nd she somehow or other fell out of bed, but apparently did not hurt herself. The nurse, however, noticed that during the night she did not move her left leg, and on examination the limb was found absolutely paralyzed. The knee-jerk at this time was quite decided on each side. She was now again very ill, the retraction of the head very marked, mental faculties much obscured, and urine repeatedly voided involuntarily.

By the middle of September she was much better again, frontal pain and vomiting much less, retraction of head much less, power returning to left lower limb, and no advance of facial paresis. On the 19th the knee-jerk was found absent from the left side, and only a trace of it was present on the right.

There were one or two occasions during November when pain in the head and vomiting were troublesome, but on the whole the patient made decided progress. She gained flesh and became bright and cheerful, though she was quite blind (optic atrophy); there was also marked horizontal nystagmus. The functions of the bowels and bladder became normal, and further, complete voluntary power was regained over the left lower limb; the evidence of right facial paresis had also receded, and the sense of smell somewhat improved.

When she left the hospital on December 4 she could stand steadily and without giddiness, and could walk with very slight assistance. Examination showed a very marked left knee-jerk, the right being both slight and sluggish.

The most impressive feature of this case was the recovery of the patient after her condition for many weeks had seemed practically hopeless. The facts of the case show the diagnosis of intracranial tumour "writ large," and evidence is not wanting to show that the situation of the tumour is probably the right hemisphere and middle lobe of the cerebellum. A combination in aggravated form of vomiting, vertigo, and optic neuritis is in itself, though not conclusive, sufficient to suggest a cerebellar growth; marked retraction of the head, and possibly nystagmus, are also not without significance in the same direction. The difficulty in maintaining equilibrium and the tendency to fall backwards indicate disturbance of the middle lobe. Paralysis of the left lower limb associated with right facial paresis shows that the pressure of the growth is mainly towards the right side, so as to involve part of the motor tract above the decussation, and the trunk of the facial nerve. This is in harmony with the usual experience of paralyses produced by pressure of a laterally situated cerebellar growth. It is noteworthy that the headache was entirely frontal, showing how little reliance is to be placed on the site of pain as indicative of the situation of the tumour. The varying character of the knee-jerks and the presence of anosmia—a rare result of intracranial tumour—are also special features of the case.

Patient was again under observation from October 10 to November 27, 1895. In the interval she had for the most part enjoyed good health, but, recently, frontal pain, vomiting and giddiness had troubled her. Under rest in bed, etc., these symptoms subsided, and she left the hospital in good general condition, and with no disturbance of gait other than might well be due to her blindness. Knee-jerks *in statu quo*. In June, 1899, patient's sister writes: "A. is never very strong, but she is free from her old symptoms, except the blindness."

The risk of recurrence of the symptoms after even a considerable period of quiescence, and the possibility of a second "recovery," are thus further features of clinical interest in the record of this case. (See also Case 5, p. 103)

CASE 2.—*Absolute blindness, with marked papillitis and subsequent optic atrophy in right eye (and presumably also in left), with history of severe pain in head, and, later, disappearance of knee-jerks; patient seen in good general health eighteen months after onset of symptoms.*

Martin T., aged 10, complaining of defective sight and of pain in head, May 1, 1898. Present illness commenced ten months ago. Pain in the head has at times been very severe, apt to come on in the evening and to keep patient from sleeping. The loss of sight has been of gradual development. At no time has it been necessary to confine patient to bed, and there have been no convulsions and no complaint of giddiness. Vomiting has occurred occasionally, but only after taking milk. No history of a fall or blow on the head.

Patient is a pale, delicate-looking boy; general physiognomy not suggestive of hereditary syphilis; he is almost absolutely blind. The left cornea is hazy from interstitial keratitis, and evidences of the same condition exist in the right eye. The pupils show very defective light-response. In the right fundus there is marked papillitis, which is showing signs of retrogression. Midway between the disc and macula is a small patch of choroidal atrophy, and to the inner side of this is a group of yellowish-white, highly refractive dots. The left fundus cannot be seen on account of the corneal haze, which, however, is not sufficient to account for the extreme visual defect.

Patient walks well and quite without ataxia; knee-jerks present but almost certainly defective. Superficial reflexes present except plantar. No evidence of disease in thoracic and abdominal viscera; urine free from albumen; some enlargement of cervical lymphatic glands.

Family History.—Father and mother alive and well; mother lost a sister from consumption. Patient is youngest of a family of eight; of these two died of "convulsions," one with "something in her brain," and one—the first-born—at seven months—cause unknown. The other children are said to be healthy (two examined show no evidence of hereditary syphilis).

Patient was kept under continuous observation for two months. At times he complained of pain in his head and displayed a very violent temper, but his appetite was good, he never vomited, and his general nutrition improved. Examined repeatedly during this period, the knee-jerks were completely absent. The only other change was the advance of optic atrophy in the right eye; the details of the left fundus could never be seen. In the beginning of December, 1898, the boy was again seen; he seemed quite cheerful and fairly vigorous. Blindness was absolute. Knee-jerks absent.

Assuming this to be a case of intracranial tumour, the only indication of the site of the growth is afforded by the loss of the knee-jerks—a fact which points with some degree of directness to the cerebellum. The probabilities are that the tumour is tubercular in nature, even though the boy is undoubtedly the subject of hereditary syphilis. Syphilitic tumours of the brain are not common in children, whereas tubercular growths are frequent, and the cerebellum is their most usual site. The family history seems distinctly tubercular. The absence of evidence of tubercle in any other part of the body—unless possibly the cervical lymphatic glands—does not mean much in a child, though in adults, according to Gowers, "the signs of phthisis are rarely absent when there is tubercular tumour of the brain." Further, the boy appeared to receive little or no benefit from antisyphilitic treatment, whilst he improved considerably under the use of iodide of iron and cod-liver oil.

Case 3.—*Double optic neuritis followed by blindness and optic atrophy; history of vomiting, with slight headache every morning, for six months preceding the loss of sight, but no other cerebral symptoms and no "illness" confining patient to bed even for a single day; knee-jerks present, though right possibly defective.*

Jas. C., aged 13; examined on April 25, 1898. His complaint is loss of sight. According to the father, the boy had perfectly good sight until the early part of January, when he fell in the street and struck the back of his head against the pavement. No apparent injury followed the fall, but in the course of a fortnight the boy complained that he "could not see well," and soon he appeared to become quite blind. It seems, however, that for six or seven months before the fall the patient was troubled with vomiting, which occurred daily, but only in the morning, and before breakfast. After each act of vomiting he had for a time some pain in his forehead, but he was always able to go out and distribute newspapers and then to return and take breakfast, and afterwards to go to school. The vomiting still continues, but is entirely confined to the early morning.

At no time has the boy been confined to bed or regarded as seriously ill, and as a proof of the comparatively recent failure of his eyesight it is mentioned that he won a prize for drawing at Christmas. In the view of the father, it is the blindness which constitutes the whole of the lad's disability.

Examination.—There is no perception of light with either eye; the pupils are dilated and have no light-response; each fundus shows the presence of consecutive optic atrophy.

He is a fairly vigorous-looking boy, quite free from evidence of hereditary syphilis. There is some enlargement of the submaxillary lymphatic glands, but no evidence of disease in the thoracic or abdominal viscera.

Knee-jerk present on each side, the right not being easily provoked and less distinct than the left (confirmed at a later date); no ataxia in gait.

Family History.—Of three children born before patient two died from "suppressed measles," the third is alive and in good health. Since the birth of patient there have been eight pregnancies, all terminating at full time—three of the children survive; the other five all died when about eight months old from "wasting," diarrhœa, vomiting, and swelling of the belly. Father and mother in good health, and with reputed good family records.

There is nothing in this case to indicate definitely the site of the tumour. Perhaps all that can be said is that the family history is marked by a number of events almost certainly tubercular, and that the most frequent situation of tubercular tumours is the cerebellum. Some difference between the degree of the knee-jerks can hardly be claimed as of definite diagnostic worth. The principal value of the case is the demonstration it affords of the great importance of giving the most careful attention to any symptom of possibly cerebral origin, and more especially if such a symptom—even though existing alone—is extended over a considerable period of time. It is practically certain that grave intracranial disease in this case produced for some considerable time no evidence of its existence other than an attack of vomiting, with some frontal headache, every morning. Then followed optic neuritis, which did not seriously affect vision until it began to pass into post-neuritic atrophy. Whether the diagnosis of tumour be justifiable or not, the practical importance of such a sequence of events as is here presented is obvious.

CASE 4.—*Serious illness at 15 years, and marked by frontal pain, vomiting, and failure of sight; four years later patient seen in good health, but the subject of post-neuritic optic atrophy.*

Rose M., aged 19, complaining of defective sight; examined August 7, 1898.

The failure of sight is referred to an illness which confined her to bed for six weeks in the summer of 1894. The symptoms of this illness were obstinate vomiting, pain in the forehead, failure of sight, and general weakness. The medical man in attendance (now deceased) is reported to have said that she had a tumour of the brain. Gradually the symptoms subsided, but the sight steadily deteriorated. No history of injury to the head. Menses have always been somewhat irregular; she cannot say there was anything unusual in this respect at the time of her illness four years ago.

Patient is a tall, healthy-looking young woman. R.V., counts fingers at 6 ft., and reads J. 16 at 8 in. L.V., hand movements only.

Each pupil is dilated and exhibits only a feeble light-response. In each fundus the disc is white, with "fuzzy" edges, arteries small, and vessels at centre of disc concealed by a filmy haze. There are distinct "white lines" along many of the vessels; at the left macula there are fine retino-choroidal changes. The knee-jerks are distinct, and the gait free from evidence of ataxia. Cardiac sounds and pulmonary physical signs normal—some, but by no means marked, evidence of anæmia.

Family History.—Father and mother alive, and in good health, Patient is fourth in a family of ten—one brother was killed in an accident, the others are all strong and healthy. She has lived all her life in a small village in the West of England.

The history and existing post-neuritic atrophy show the three symptoms, viz., headache, vomiting and optic neuritis, which, to quote Hughlings Jackson, "make the diagnosis of an 'adventitious product' within the skull almost certain." That the facts justify such a diagnosis in the case of a patient apparently exceptionally favoured as regards hereditary and other influences, seems most unaccountable. The pathetic spectacle of a seemingly healthy and vigorous girl reduced to hopeless blindness may well emphasize the urgency of every possible measure which may conceivably help to avert so disastrous an issue.

CASE 5.—*Evidence of severe intracranial disease of three months' duration in boy aged 7, followed by recovery, with the exception of blindness (optic atrophy) and loss of knee-jerks; return of headache, vomiting, etc., with a fresh attack of papillitis after an interval of three years; repeated subsidence of all symptoms of active disease, and boy six months later known to be in good general health.*

Jno. D., aged 10, seen in December, 1896. He is reported to be blind, but is otherwise in good health. Some eighteen months ago he had a very serious illness, being confined to bed for nearly three months, and at times he was thought to be dying. The most troublesome symptom of this illness was vomiting; he also had pain in his forehead, his head was drawn backwards, and his sight failed. The diagnosis at the time was "tumour of the brain." He is now a plump, cheerful-looking boy. He is quite blind, and the pupils have no light-response; in each fundus there is decided evidence of a former papillitis, now in the atrophic stage. Knee-jerks absent, gait normal, memory and mental condition generally seem quite acute. Thoracic and abdominal viscera healthy.

The boy is said to have had a brick fall on his head many years ago.

July, 1898. The boy has been in good health until six weeks ago, when vomiting returned, and has continued until the last four days; there has also been pain in the head (frontal); at times he has seemed to be unconscious, and on two occasions has had a "fit," in which he became "stiff," but not convulsed. He is now much better in every respect, and indeed he says he is quite well. The ophthalmoscope shows the addition of a recent papillitis to the atrophic condition formerly noted. Knee-jerks are absent.

In January, 1899, the boy is reported to be "stouter and more cheerful than ever." The family history shows a twin brother and three younger brothers and sisters alive and well. Father and mother also enjoy good health; no knowledge of family liability to chest or other disease.

This case, like Case 1, shows how liable the symptoms of intracranial disturbance are to return even after a considerable period of quiescence. Every care should therefore be taken to reduce the risk of this possibility by maintenance of the general health and by the use of such agents as cod-liver oil, iodide of iron, etc.

There is one feature attending the recrudescence of the symptoms which is very unusual, viz., a second attack of papillitis in discs atrophied as a result of the first attack. Gowers, in his work on "Medical Ophthalmoscopy," writes: "If a disc has become completely atrophied it is very rarely again the seat of inflammation. When atrophy is partial or absent, in rare cases two attacks of neuritis may occur." Cases of second attacks of papillitis following complete optic atrophy have, however, been observed by Dr. Hughlings Jackson and by Schweinitz.

Case 6.—*Blindness (post-neuritic optic atrophy) and occasional quasi-epileptic attacks, but otherwise good health, in a girl aged* 16, *who, two years before, was for several months seriously ill with symptoms of active intracranial disease.*

Ada L., aged 16; examined on June 20, 1898. She complains of blindness. Patient is a delicate-looking girl. The pupils are dilated and have only a slight response to light; they move freely in convergence. Each fundus shows evidence of a former neuro-retinitis with marked atrophy of the optic papilla. Blindness appears to be absolute. The cardiac sounds are pure; R.M. over lungs abundant and free from râle; nothing noteworthy in physical examination of abdomen.

The first note on the knee-jerks is that they are "present," subsequently the right one is "hardly obtained," and the left elicited only with difficulty. The gait is free from ataxia.

From May to November, 1897, patient was very ill, suffering from attacks of giddiness, severe pains in the head (which was "drawn back"), and gradual failure of sight. In the course of this illness she had two "fits," attended by unconsciousness and followed by "paralysis of one arm and leg." No history of injury to head or of discharge from either ear. With the exception of the defect of sight, she has been improving since the beginning of the year, and is now able to walk about undisturbed except by an occasional pain in the head; there is no sign of hemiplegia. Her mother adds that patient at times has some kind of "fit" or "attack" which she cannot well describe. There is no loss of consciousness and no convulsion, but patient calls out "I am worse," and clutches hold of anyone who may happen to be at hand. Some of the attacks occur at night-time, and on several occasions there have been involuntary evacuations.

Family History.—Father and mother enjoy good health; the latter says a number of her relatives in the earlier generations died of consumption. There are three other children besides patient, and these are alive and well. No deaths; no miscarriages.

CASE 7.—*Blindness and bilateral optic atrophy without evidence of previous neuritis or papillitis in girl aged* 21; *other evidences of (presumably) intracranial disease being nystagmus, quasi-epileptic seizures, explosive temper, and absence of knee-jerks. History quite free from evidence of any acute cerebral attack,* *the loss of sight occurring gradually at* 11 *years, and unaccompanied by any other symptom of intracranial or other disease.*

Edith H., aged 21; examined on July 4, 1898. Her statement is that she is quite blind.

Until 11 years of age she had good sight and was in every respect strong and well. Then gradual loss of sight came on, and slowly progressed until in the course of twelve months or less she became quite blind. The first sign of the defect was that the girl made mistakes in reading and often fell over objects when walking. At no time did she complain of pain in her head or of vomiting. (Pressed on this point, she says that about a year ago she often used to vomit after breakfast, but with this exception she has never been troubled with vomiting.) She is sure that the failure in her sight was not accompanied by any general illness, and she was never confined to bed. No history of injury to head. For the last three years she has been subject to occasional fits in which she "gets stiff but does not fall, and is unconscious for a minute or so." She is liable to outbursts of temper. But her general health is and always has been very good; menses quite regular during last five years. Patient is a healthy-looking woman. She is quite blind, and the pupils, which are widely dilated, have no light-response. The optic disc on each side is very white, and the retinal arteries, and perhaps also the veins, diminished in calibre. Neither in the disc itself nor in the fundus generally is there evidence of a previous neuro-retinitis. There is marked horizontal nystagmus (present for five years). Knee-jerks absent; no ataxia in gait. Examination of chest and abdomen entirely negative.

Father, mother, three other members of family—older than patient—have quite good sight and satisfactory health. No knowledge of fits, nervous disease, or defective sight in any relatives.

The reasons for placing this case as one of intracranial tumour have already been advanced (p. 75). The failure of sight without other suggestion of intracranial mischief is remarkable. The case is another example of grave intracranial disease existing for a long time and yet producing, apart from blindness, only slight signs of its existence.

CASE 8.—*Paralysis of sudden onset of right external rectus, also double optic neuritis, but no other evidence of cerebral disease, and no considerable visual defect other than diplopia; patient, a girl aged 17, somewhat anæmic; but otherwise physical examination normal. Recovery complete and apparently permanent.*

Maud E., aged 17, cook, complaining of double vision (June 7, 1898). The diplopia is of a fortnight's duration; it was of sudden onset, a group of children at whom patient was looking "suddenly becoming double." She has no other ground of complaint. Patient has always been healthy, though she admits some recent loss of colour, breathlessness on exertion, and unduly long inter-menstrual intervals. She was menstruating at the time the diplopia developed.

There is almost complete paralysis of the right external rectus muscle, and homonymous diplopia increasing to the right; she also recognizes some increasing separation of the images to the extreme left, but there is no appreciable defect in the action of the left external rectus. No ptosis or other defect of ocular movement, and pupils equal and with normal reactions.

R.V. 6/6 .5 Hm. J. 1 at 5 inches.
L.V. 6/6 .5 Hm. J. 1 at 5 inches.

There is marked optic neuritis in each eye—the disc is moderately swollen, with numerous hæmorrhages on the surface of the swelling; the vessels as they leave the disc are concealed by exudation, and the veins generally are enlarged and tortuous.

The visual fields for white and colours are normal, and there is no central or other scotoma.

Physical examination reveals nothing beyond a venous hum at the root of the neck and a blowing murmur with each expansion of the subclavian arteries. Urine free from albumin and sugar.

She has but an incomplete knowledge of her family history; all she can say is that her mother died of "consumption and heart disease."

Patient ordered to take complete rest, and iron prescribed.

June 21. General condition improved; papillitis somewhat less marked; she does not read 6/6 fully with either eye; paralysis of right external rectus less complete.

July 25. Improvement still more manifest. There is now only a slight degree of paralysis of external rectus. With the right eye she makes mistakes in reading both 6/9 and 6/6, and with the left does not read 6/6 fully. There are now definite changes at each macula; a circular area of a deep red colour and about the size of the optic disc occupies the centre of each fundus, and on this, more especially in the right eye, are a number of irregular, whitish, lustrous spots. Papillitis subsiding.

August 2. The patient is looking very well. There is now a hardly appreciable defect in the outward movement of the right eye, but homonymous diplopia can still be detected to the extreme right when the flame and coloured glass are used. The greater part of the margin of each disc can now be seen; each fundus otherwise unchanged. Visual acuity as before; recognizes all colours in 1 mm. square in central vision.

August 30. Improvement continues. The discs are nearly normal and the congested areas in the macular regions less prominent. There is no diplopia even to the extreme right.

September 20. Patient regards herself as quite well, and appears to be so. The fundus on each side remains as before, and there is slight defect in the visual acuity of each eye.

She is allowed to return to work: Is to continue to take iron for another three months.

March 30, 1899. In good health. Evidences of former neuritis readily appreciable in each fundus, but vision of normal acuteness.

The difficulties of diagnosis in this case have already been discussed (p. 82). As the patient was undoubtedly anæmic, it seemed right to order complete rest and the administration of iron. Had there not been decided improvement under this treatment it would have been wise to have tried mercurial inunction (see Case 9).

CASE 9.—*Rapid failure of vision in girl aged 14, with marked double papillitis not accompanied by headache, vomiting, or any other suggestion of cerebral disease; prompt improvement under mercurial inunction, the visual acuity reaching almost to the normal level within three months. Patient after an interval of nine months found to have nearly full vision, normal visual fields, and good general health.*

Ellen D., aged 14, seen on June 15, 1898, complaining of failure of sight in each eye. She could see quite well until a week ago; at that time she had a shooting pain in the right eye, and the sight of the eye rapidly failed. A similar experience soon occurred in the left eye. No pain in the head and no vomiting, and general health quite unaffected.

R.V., denies even perception of light. L.V., hand movements only. Pupils dilated: right no response to direct light, but contracts on consensual stimulation; left, considerable light-response, but contraction not well sustained. No ophthalmoplegia externa. Right fundus shows decided papillitis (fully +3 D), with a number of hæmorrhages on the surface of the exudation; similar but less marked condition in left eye.

Movement of the eyes in any direction does not cause pain, and palpation of the globes scarcely causes complaint.

Patient is well nourished; no appearance of anæmia; physical examination of chest and abdomen normal; urine normal. Knee-jerks normal, no ataxia in gait, superficial reflexes easily obtained, cutaneous sensibility and motor functions undisturbed. At no point is the skull tender to percussion. She has never been very strong; had rickets in infancy, jaundice four, and measles two years ago. No history of injury to head or of discharge from ears. She has never menstruated.

Family History.—Father alive and well; his mother and a sister and brother died of consumption; mother lost a brother from the same disease. Patient is the eldest of a family of four—one died of consumption, the other two are "delicate."

July 6. Mercurial inunction has been used every night. She can now count fingers in the peripheral part of the field of each eye, but not with central vision, and can move about the ward without stumbling against objects as she did on admission. The papillitis has distinctly subsided.

August 6. Has been away at seaside. Vision still improving. R. counts fingers at 3 ft. in central vision; L=6/24. Contraction of pupils under light much more marked, but that of right is not well sustained. Discs white and filled in.

August 13. R.V.=6/18; L.V.=6/12 (less one letter).

September 3. R.V.=6/9 (two letters); L.V.=6/9 nearly. Pupils not definitely abnormal.

September 17. R.V. 6/6 part; L.V. 6/6 part. Fields full, no central scotoma, recognizes all colours through an opening 1 mm. sq. Whiteness of discs very decided, and arteries look small.

June, 1899. Patient is in good health, and the visual acuity is almost, though not quite, up to the normal level. Objective examination of the eye negative, except for evidences of former papillitis.

It is suggested that this is a case of retro-bulbar neuritis affecting both sides, and with a greater degree of papillitis than is usual, and the reasons for proposing this diagnosis are given on p. 86.

ON THE CEREBRAL AND OCULAR COMPLICATIONS OF ANÆMIA AND ON THE PROBABLE RELATION OF THESE TO THROMBOSIS.[1]

MR. PRESIDENT AND GENTLEMEN,—I have the honour to offer to the Society some observations on the cerebral and ocular complications which occur as occasional events in the clinical histories of anæmic women, and I propose to maintain, subject to your better judgment, that the interpretation of these events is to be found in the intravascular formation of blood-clot. That among the special characters which pertain to the anæmic state there must be included an undue tendency to blood-coagulation is generally admitted. At least, this is true so long as the blood is contained within the blood-vessels. When, on the other hand, anæmic blood is withdrawn from the vessels, the opposite condition obtains, and clotting is both delayed and incomplete. Upon what exactly this difference depends it may be difficult to say, but such an experience certainly suggests that intravascular coagulation in anæmia is dependent less on some particular quality of the blood than on an abnormal state of the circulation; and seeing that such clotting is far more frequent in venous than in arterial channels, and that it is mainly exhibited in situations where, for anatomical reasons, the movement of the blood is comparatively slow, the suggestion may be accepted that an important or determining factor in the thromboses of anæmia is a delayed or sluggish rate of blood-flow. There are, however, quite a number of rival hypothetical explanations of the occasional occurrence of thrombosis in anæmic patients, but none of them would appear to have secured anything like unanimous acceptance. In connection with this communication it is not necessary that these explanations should be discussed; nor is it necessary to inquire why, although thrombosis is in exceptional instances a prominent feature in anæmia, the enormous majority of the patients who suffer from this condition are entirely free from all clinical evidence of intravascular clot formation. For the most part, indeed, anæmia, at least as seen in the shape or fashion of chlorosis, pursues a course so uniform and so undisturbed that even a considerable clinical experience may fail to secure a personal introduction to any development which owes its existence to thrombotic or other exceptional incidents. Yet it is beyond dispute that serious and even fatal complications sometimes occur in anæmic women, and it is not less certain that the explanation of these is to be found in clot formation within the blood-vessels. Hence, whatever be the exact pathological interpretation of this event, or of its restriction to a relatively small number of patients, it is necessary to accept thrombosis as an occasional accident in the course of anæmia, and to be prepared, both as an exercise in diagnosis and as a practical responsibility in regard to treatment, to deal with the situations thus created.

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[1] An Address delivered to the Medico-Chirurgical Society of Glasgow, and reprinted from the *Lancet* of September 19, 1908.

One of the objects of the present communication is to suggest that a number of comparatively minor disturbances occasionally observed in anæmic women, as well as certain acute and serious complications of anæmia, owe their origin alike to intravascular blood-clotting. Such disturbances are perhaps more frequent than is generally allowed, and if the proposition that they are due to thrombosis be established, they become of considerable clinical importance. For, they then appear as warnings or danger signals of the possibility, or even of the probability, of clot formation on a large scale or in particularly dangerous situations, and they thus claim the special therapeutic measures by which results so disastrous may be averted.

In short, the argument here to be presented is, that in thrombosis may be found an explanation both of various grave, urgent, and well-recognized complications of the anæmic state, and also of certain relatively slight disturbances which appear as occasional departures from the usual course of anæmia as this exists in its simpler and less serious types.

Probably the most frequent and obvious exhibition of thrombosis in anæmia is met with in the large veins of the lower limbs. The symptoms include pain, tenderness, and œdema; and there may or may not be febrile temperatures. The thrombosis may extend from the femoral to the iliac veins, and even to the inferior vena cava; and in occasional instances it would appear that the process has been limited to these larger channels. It is rare for the veins of the upper limbs to be involved, but they are not wholly exempt; and the same is true of the large veins of the neck and chest. Of course, in all such cases there is a risk of pulmonary embolism, and the whole of the facts in this connection have been repeatedly verified by post mortem examination. Apart from embolism, however, the prognosis in the venous thrombosis of anæmia is far from gloomy, and it is of interest to note that the circulation in the obstructed veins may be completely restored. This is shown, not only by the disappearance of dropsy, but also by the recession of a collateral circulation, which, as a compensatory process, may have become established in superficial venous channels. Hence it must be accepted—and the experience is of importance in connection with the argument here to be advanced—that an obstructing clot formed in a vein in association with anæmia, may, presumably by absorption or organization, be so completely removed as to cease to exercise any disturbing effect on the normal flow and movement of the circulation. Such a satisfactory issue has been observed even when the thrombotic process has been so widely spread as to affect the majority of the large veins both of the trunk and limbs.

The next situation to be recalled in connection with the occurrence of thrombosis in anæmia is the intracranial veins and sinuses. Every now and then there is recorded in the medical journals the sudden development of acute cerebral symptoms in a young woman, who, save for the existence of anæmia, had previously been regarded as entirely free from evidences of organic disease. The symptoms include severe headache, vomiting, pain in the neck, retraction of the head, stupor, delirium, various paralyses, and sometimes double optic neuritis; and in not a few of the cases there has been a fatal issue. In view of these symptoms, the diagnosis of intracranial tumour, or, when, as is sometimes the case, there are febrile temperatures, of meningitis, can hardly excite surprise. Yet there can be no doubt that most, if not all, of these cases are examples of venous thrombosis within the skull. This position is fully justified by the experience that in not a few instances post mortem examination has demonstrated the presence of thrombosis in the intracranial veins and sinuses, and has at the same time failed to detect any other lesion capable of explaining either the symptoms or the fatal issue. There are other records, in which, with similar symptoms, recovery has followed, and here the diagnosis of intracranial thrombosis has sometimes been supported by the development of thrombosis in other situations, as, for example, in the jugular or femoral veins.

Upon this order of cases two observations may be offered. The first is, that as in thrombosis affecting the veins of the limbs, both the obstructing clot and the consequences which attend such obstruction, may, in a clinical sense, entirely disappear, the fact of complete recovery from an acute cerebral attack is not necessarily in conflict with a diagnosis of intracranial thrombosis as the cause of such an attack. If the circulation may be restored after thrombosis of, say, the femoral vein, a similar result may surely follow thrombosis of the cerebral veins and sinuses. Thus, while widespread clot formation in this latter situation may perhaps produce a fatal result, a less extensive thrombosis may well be attended by clinical disturbances from which a complete recovery is possible. Hence, recovery, whether from a group of symptoms, or, perchance, from a single symptom, of manifestly cerebral origin, is entirely consistent with a diagnosis of thrombosis affecting the intracranial veins and sinuses.

The second remark submitted in connection with severe or fatal cerebral attacks occurring in anæmic women, is, that such cases may include among their symptoms the presence of double optic neuritis. If, therefore, these cases are due to thrombosis in the venous channels of the skull—and escape from this conclusion seems impossible—it follows that such thrombosis is an efficient cause of double optic neuritis. It is true that in some of the cases to which reference is now made optic neuritis has not been present. But these negative experiences cannot invalidate the definite and positive observations in which optic neuritis, existing together with other cerebral symptoms, has been found to have no pathological association other than intracranial thrombosis. The position finds an exact parallel in intracranial tumour. In some cases of tumour double optic neuritis is a prominent feature; in others the optic discs are normal. No one will use this latter fact as an argument to contest the proposition that intracranial tumour is competent as a cause of double optic neuritis. And the same conclusion must be applied to intracranial venous thrombosis. Among its possible effects, therefore, must be accepted the development of double optic neuritis.

It is hardly necessary to argue that another consequence of such thrombosis may be a limited paralysis—say, for example, of one or other of the ocular muscles. If hemiplegia may be due to intracranial venous thrombosis—and this is beyond dispute—an ocular paralysis may well own a similar explanation, and indeed, there are many cases in which this association has been clearly established. Therefore it may be affirmed (1) that intracranial sinus and venous thromboses are the cause of the more or less severe cerebral attacks which occasionally complicate the course of chlorosis; and (2) that such thromboses may include among their consequences both double optic neuritis and ocular or other limited palsies.

With these propositions admitted, attention may be directed to the following summarized case-record:—

Maud E.,[1] aged 17, manifestly the subject of chlorosis, came to hospital complaining of double vision. Examination showed that the diplopia was due to paralysis of the right external rectus muscle, and that each optic disc was swollen and its edges concealed by exudation; there was also some retinitis. No albuminuria or evidences of organic disease other than the above were present. In the course of three months the patient had made a complete recovery.

Here, then, is a patient whose illness conforms strictly to the usual course of chlorosis, except for the fact that she has double optic neuritis and an ocular paralysis. These two events, if the argument already advanced is sound, may certainly be accounted for by intracranial thrombosis, to which, from her anæmic condition, the patient is undoubtedly liable. An alternative explanation is hard to suggest, and in view of all the circumstances the diagnosis of intracranial thrombosis may reasonably be held to be probable or even inevitable.

Take a second case-record.

Mary J.,[2] aged 22, admitted to hospital on account of severe headache, breathlessness, double vision, and failing sight. She was found to be the subject of chlorosis, of marked double optic neuritis with retinitis, and of paralysis of the left external rectus muscle. In addition, the knee-jerk could not be obtained on either side. The patient ultimately made a complete recovery, though the return of the knee-jerks was delayed for many months.

Here again the argument in reference to the cause of the optic neuritis and ocular paralysis may be repeated. Thrombosis will explain these facts; it will also, doubtless, explain the severe headache. Possibly the loss of the knee-jerks may not so readily accommodate itself to the diagnosis of intracranial thrombosis, but even here it may be remembered that certain intracranial tumours, and also some forms of meningitis, cause a similar loss. Further, in some of the more serious cases of anæmic thrombosis as proved by necropsy, distinct disturbance of the tendon-jerks has been observed, though this has perhaps been rather in the direction of excess than defect. Altogether there seems abundant justification for the claim that the above case is a further example of double optic neuritis and an ocular paralysis occurring in a patient free from all evidence of disease other than anæmia, and that these two symptoms must find in intracranial thrombosis their clinical and pathological explanation. Hence it would seem that not only may double optic neuritis and an ocular paralysis be numbered among the clinical events possibly attendant upon intracranial thrombosis, but also that they may appear as the only evidences of this condition. This conclusion brings such case-records as the two just quoted into a natural relation with the more extreme examples of cerebral disturbance to which reference has already been made, and in a certain number of which intracranial thrombosis has been demonstrated on necropsy. The suggestion, in short, is, that the difference between the two groups is merely one of degree, and that both groups own a common cause in the shape of thrombosis affecting the cerebral veins and sinuses.

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[1] Reported in detail on p. 107.

[2] See article by Mr. Ernest Clarke and Dr. Hawthorne, *Lancet*, April 30, 1904, p. 1198.

This position is further strengthened by the knowledge that experiences intermediate between the two extremes are by no means wanting. There are well-authenticated cases of anæmia in which the patients have been attacked more or less suddenly by severe headache, pains in the neck, and vomiting, together with optic neuritis and ocular paralysis. It is common to suggest that the intracranial condition responsible for these symptoms is a localized and transient meningitis, or an œdema of the brain, though positive evidence in support of either of these views is difficult to find. On the other hand, thrombosis will cover all the facts, and, if allowed, will permit the construction of a graded series of cases, occurring in anæmic patients, characterized by more or less severe evidences of cerebral disturbance, and finding in intracranial blood-clotting a common bond of interpretation. At one extreme are fatal cases with such symptoms as coma, delirium, various paralyses, and sometimes optic neuritis, and proved by necropsy to be dependent on intracranial thrombosis; next come examples of less severe but still considerable cerebral disturbances, including optic neuritis, and having a favourable termination, the diagnosis of intracranial thrombosis being confirmed by the occurrence of thromboses in the jugular, femoral, or other veins; succeeding these may be placed cases of unusually violent headache and vomiting, with optic neuritis and an ocular paralysis; and after these fall those cases of anæmia in which optic neuritis and an ocular paralysis are the sole unusual events. The earlier members of this series are demonstrably dependent on thrombosis, whilst, as has already been argued, the cases in the last group readily adapt themselves to the same explanation. To detach the intermediate members of the series from the diagnosis of thrombosis is surely a somewhat arbitrary and unnatural proceeding, more especially when any suggested alternative explanation cannot possibly be more than a mere surmise. Hence the claim may be pressed, that in all the groups above presented as examples of more or less severe cerebral disturbances complicating anæmia, there is one and the same underlying pathological condition—namely, thrombus formation in the intracranial veins or sinuses.

The question now arises whether the series of cases to be explained in this fashion can be carried a stage further. If optic neuritis and an ocular paralysis appearing together in a case of anæmia are to be attributed to intracranial thrombosis, is it not probable that when, under similar conditions, either exists alone, thrombosis is again the responsible cause? Thrombosis is competent to cause each of these events; it is also a recognized complication of the anæmic state; further, it may be questioned whether any more satisfactory, or indeed any equally satisfactory, explanation can be advanced.

Hence thrombosis has a fair claim for recognition as the cause both of optic neuritis and of an ocular paralysis when one or other of these symptoms appears as the sole unusual departure from the ordinary clinical course of simple anæmia. Ocular paralysis, it may be admitted, is only very occasionally in this position. Double optic neuritis, on the other hand, is a well-recognized complication of anæmia, and its occurrence has given rise to a good deal of speculation. Here it is presented—and the same may be said of an ocular paralysis—as the simplest clinical expression of an intracranial thrombosis, and the claim is made that an examination of the series of cases in which cerebral events complicate anæmia, affords a presumptive proof of the correctness and adequacy of this proposition.

An attempt may now be made to find support for the argument that optic neuritis in anæmia is probably due to intracranial thrombosis, by an examination of certain other conditions in which, without any very obvious explanation, optic neuritis occasionally appears. One of these is the acute anæmia following a free hæmorrhage, as from the stomach, intestine, or uterus. Standing alone, such cases have a very mysterious aspect. It will, however, be allowed that the condition of anæmia established by hæmorrhage is one in which thrombosis may readily occur, as indeed is occasionally demonstrated by clot formation in the veins of the trunk or limbs after considerable loss of blood attending surgical operations. Hence it is not impossible—not even improbable—that free bleeding from a mucous surface may be followed by intracranial thrombosis, and such thrombosis may be advanced as the immediate cause of the optic neuritis in the cases now under consideration. It is at least suggestive that optic neuritis sometimes occurs in circumstances in which intracranial thrombosis is at least a possibility, and where an explanation of the neuritis other than thrombosis is not easily imagined. Thus the occasional occurrence of optic neuritis in the acute anæmia following free hæmorrhages from mucous surfaces may be quoted in support of the doctrine that the optic neuritis sometimes seen in chlorosis is dependent on intracranial thrombosis.

Another condition in which optic neuritis may appear without any very manifest explanation is suppurative disease of the middle ear. In some instances of this disease optic neuritis is associated with an ocular paralysis, and in others an ocular paralysis exists without any attendant optic neuritis. That is, purulent disease of the middle ear may present ocular complications identical with those occasionally found in simple anæmia—namely, optic neuritis and ocular paralysis—and these two events may be associated, or either of them may exist apart from the other. So far as optic neuritis is concerned the suggestion has been made that this may complicate tympanic disease altogether apart from intracranial complications. Obviously this can only mean apart from intracranial complications producing manifest cerebral symptoms, and the suggestion by no means guarantees the absence of sinus thrombosis, to which purulent otitis media is notoriously liable. For the most part, however, an optic neuritis, and perhaps still more an ocular paralysis, developing in the course of suppurative disease of the middle ear, means, without question, some more or less serious development within the skull. This development may be abscess, meningitis, or sinus thrombosis, and to judge from post mortem records it would appear that of the three it is thrombosis which is most likely to give rise to optic neuritis.

Further, on more than one occasion it has happened that removal of clot from the lateral sinus has been followed by disappearance of an existing optic neuritis, and although in those cases where the patient has recovered it is impossible absolutely to exclude meningitis, the absence of evidences of this condition has repeatedly been noted during the operation, while, still more convincing, in some of the fatal cases necropsy has demonstrated that meningitis was not present, and that sinus thrombosis, perhaps with more or less œdema of the brain, was the only abnormal intracranial condition that could be detected. Again, it has on several occasions been found that optic neuritis—and sometimes, too, an ocular paralysis—has existed in middle-ear disease quite apart from any other evidence of cerebral disturbance, and yet has disappeared, either without operation, or after operative measures restricted to the mastoid and tympanum and not involving the cranial cavity. Obviously, in these cases, there cannot have been a cerebral abscess, and meningitis is improbable, or at least is less probable than thrombosis. Among the recognized possibilities of the situation, therefore, there remains only thrombosis, and this, as shown in the records already quoted, is fully competent to explain the ocular conditions to which attention is now directed. In short, both optic neuritis and ocular paralysis may complicate middle-ear disease, and these complications may be explained by intracranial thrombosis, to which such disease may most certainly give rise. In certain fatal cases no intracranial condition competent to explain these events other than thrombosis has been discovered. Is it not, therefore, in the highest degree probable that when, either optic neuritis, or an ocular paralysis, occurs as the single unusual event in the course of purulent disease of the middle ear, that sinus or venous thrombosis is the responsible cause? And if this is true of middle-ear disease, is not a similar conclusion inevitable in reference to anæmia? In both diseases thrombosis is a possibility; in both, such thrombosis occasionally produces severe cerebral disturbances, and these may include either optic neuritis or an ocular paralysis; and in both, each of these last-mentioned incidents may exist as the sole unusual event. To find, apart from thrombosis, an element common to the two diseases, and able to account for optic neuritis or an ocular paralysis existing as an isolated occurrence, is a task not easily accomplished, and it may therefore be urged that there is at least a strong presumption that intracranial thrombosis is in each instance the correct explanation.

Here, then, are three conditions, namely, acute anæmia, middle-ear disease, and chlorosis, in each of which optic neuritis apart from other complications occasionally makes its appearance. In each there is a possibility of intracranial thrombosis, and this must be allowed as at least a possible cause of the neuritis. To suggest some other cause, capable of explaining the neuritis, and at the same time common to the three diseases now in question, is a demand not readily satisfied. Hence it may be claimed that the study of the clinical history of acute anæmia and of purulent disease of the middle ear, adds force to the contention that the optic neuritis sometimes seen in anæmic women is dependent on the occurrence of thrombosis in the veins and sinuses within the cavity of the skull.

From all these experiences it might perhaps be concluded that the suggestion of intracranial thrombosis as the cause of the optic neuritis of anæmia is so strongly supported that it needs must command universal assent. This, however, is far from being the case. On the contrary, the proposal excites both incredulity and the opposition of rival interpretations. At the outset criticism applies to it this censure—that as there is no known or conceivable link by which thrombosis as a cause can be connected with optic neuritis as a consequence, the suggestion that the one is the cause of the other is not tenable. To this it may be answered that failure to supply an exact explanation of the mechanism uniting two associated events, cannot be successfully pleaded against the proposition that one of these is the cause of the other, provided there are, otherwise, valid reasons which render such a relation probable. Were such a canon of criticism enforced it would, to quote but a single illustration, compel a refusal to accept intracranial tumour as a cause of optic neuritis, for the pathway leading from the one to the other is far from apparent. Yet, while claiming the protection of this general proposition, it may be advisable to attempt to find some apt and suggestive reasons on which a belief in the capacity of thrombosis to produce such consequences as optic neuritis and ocular paralysis may be reasonably based. Regarding the latter there can be no difficulty. Indeed, it is universally recognized that thrombosis in certain of the sinuses at the base of the skull does, as a matter of experience, and presumably by pressure on the trunks of the cranial nerves, cause paralysis of the muscles of the eyeballs. Perhaps the most obvious examples of this condition are those in which the thrombosis involves the cavernous sinus, and where the resulting paralysis appears in the distribution of the third and sixth nerves. But by extension of the thrombosis into, say, the petrosal sinuses, nerves other than the two just mentioned may also be implicated, and in this way a comparatively wide area of paralysis may be produced, presumably by the pressure of blood-clot. Again, thrombosis must be considered in connection with the disturbances likely to occur in the tissues in the immediate neighbourhood of the obstructed vessel. These disturbances include such events as hæmorrhage, œdema, and softening, any one of which, if favourably situated, may readily interfere with the functions of the centre or trunk of one or other of the cranial nerves.

Possibly the considerations just advanced, though plainly applying to ocular paralyses occurring in anæmia, may be said to leave untouched the issue in regard to optic neuritis. Yet even here the chance of finding an explanation is not altogether hopeless. A thrombus, particularly if it is formed in one of the larger sinuses, may be regarded as a "foreign body" or "adventitious product" within the skull, and in this sense comes under Hughlings Jackson's definition of intracranial "tumour." So presented it appears as a not improbable cause of optic neuritis, or at least the connection between it and optic neuritis is hardly more mysterious than is the connection between optic neuritis and any other form of "tumour." With this suggestion, too, must be taken the probability that sinus thrombosis pure and simple is very much less likely to be accompanied by general symptoms of cerebral disturbance than is thrombosis affecting the cerebral veins. Indeed, there can be little doubt that a limited sinus thrombosis may occur entirely without cerebral or other symptoms. On the other hand, it is in cases where the cerebral veins are involved—and especially where they are extensively involved—in the thrombotic process, that severe and general symptoms display themselves. Hence there is no difficulty in imagining in a case of anæmia a sinus thrombosis sufficient to cause such a limited clinical expression as optic neuritis, and yet insufficient, in consequence of non-extension of clot formation to the neighbouring veins, to excite any general exhibition of cerebral disturbance. Such a suggestion, it may be held, provides a possible explanation of optic neuritis appearing in anæmia as an isolated event, and also meets the objection that, were the cause of such neuritis thrombus formation within the skull, there must needs be clinical evidence of this on a more or less extensive scale. In any event, whether the proposals here advanced on the speculative aspects of optic neuritis and ocular paralyses as clinical developments in certain cases of anæmia, are, or are not, warranted, it is beyond question that intracranial thromboses may and do occur in anæmia, and that among the clinical evidences of such thromboses optic neuritis and ocular paralyses are most unquestionably to be numbered.

Again, it has been urged that if thrombosis of the veins and sinuses within the skull can cause optic neuritis, the same result ought to follow blocking, as by thrombosis or embolism, of the cerebral arteries; and this admittedly (unless the clot be septic) is not the case. With all deference, the relevance of this argument may be contested. The circulatory and other changes which follow obstruction of an artery are not identical with those produced by obstruction of a vein, and in view of the admitted obscurity of the relations between optic neuritis on the one hand and intracranial lesions on the other, it is hardly safe to conclude that because arterial thrombosis does not cause optic neuritis venous thrombosis must necessarily fail to do so. Against such a conclusion, further, may be set the facts of experience, which compel the conviction that between venous thrombosis and arterial thrombosis there is, so far as optic neuritis is concerned, some essential difference. The one, whatever be the explanation, is, or may be, in the clinical order of events, associated with optic neuritis; the other has no such association. In view of such facts it seems idle to argue that because arterial obstruction does not cause optic neuritis venous obstruction must also be incompetent in this respect. The facts show the contrary. *Hippocrate dira ce qu'il lui plaira; mais le cocher est mort.*

Once more, it has been argued that whereas the optic neuritis of anæmia commonly subsides as the anæmia disappears under the administration of iron, the neuritis must be due to some depreciation in the quality of the blood; were the neuritis due to thrombosis, the therapeutic use of iron could not have any effect on it. The answer to this is that the optic neuritis of anæmia, even when iron is prescribed, by no means always disappears promptly, and that its movement is far from keeping strict time with the improvement in the blood quality. On the contrary, the neuritis may persist for weeks or sometimes even for months after all the ordinary evidences of anæmia have completely disappeared. Further, it must be accepted that optic neuritis—and the same is true of the evidences of thrombosis generally—is not confined to cases in which the degree of anæmia is extreme. It may, indeed, exist with but moderate evidences of anæmia, and is frequently absent from the more severe cases—experiences difficult to reconcile with the view that the neuritis is due directly to some blood defect. On the other hand, the suggestion of thrombus formation and persistence is readily consistent with an optic neuritis which endures in spite of a cure of the anæmia, while the ultimate disappearance of the neuritis may reasonably be attributed to removal of the thrombus by those natural processes under the operation of which the channel of a thrombosed vessel may be completely restored.

Thus, to summarize the objections, it may be said that neither a difficulty in providing a manifest explanation of the mechanism by which sinus thrombosis may produce optic neuritis; nor the claim that the frequent absence of evidence of general cerebral disturbance with such neuritis opposes the diagnosis of intracranial thrombosis; nor the observation that in arterial thrombosis optic neuritis does not occur; nor the comment that the neuritis disappears under treatment by iron—is found, on examination, to present any insuperable objection to the proposition that optic neuritis occurring in anæmia has, in all probability, its explanation in the formation of blood-clot in the veins or sinuses of the brain.

Attention may now be directed to certain other hypotheses which have been advanced to explain the occasional existence of optic neuritis in anæmia. One of these suggests that such cases are really examples of poisoning, either by lead or syphilis, and that to one or other of these agents both the anæmia and the neuritis must be attributed. It may be said at once that there is little, if any, positive evidence to support this statement. The recorded cases of optic neuritis in association with anæmia do not exhibit any of the usual symptoms either of plumbism or of syphilis, and the cure is effected, not by remedies appropriate to these diseases, but by the direct treatment of the anæmia. It may also be remarked that optic neuritis as it occurs in anæmia is frequently marked by very considerable swelling of the optic discs, and is on this account much more suggestive of some definite intracranial lesion than of a toxic condition of the blood, in which, when it produces optic neuritis, the swelling of the discs is a comparatively insignificant fact. Indeed, the ophthalmoscopic picture in the optic neuritis of anæmia often exactly agrees with the facts common in intracranial tumour, and unless this is remembered a serious error in diagnosis may be made, especially if, as is not uncommon in anæmic women, the patient also suffers from more or less severe headache and from occasional attacks of vomiting. (Gowers.)

According to a second proposal, the optic neuritis of anæmia depends on œdema of the brain. This, however, is little more than an empty phrase. The only evidence in support of such a view is, perhaps, to be found in some few cases in which the existence of optic neuritis in association with amenorrhœa has been recorded, the necropsy revealing an excess of fluid in the cerebral ventricles. Such a pathological condition can, however, hardly have been a primary one, and in the absence of any definite statement in reference to thrombosis it seems not unlikely that the cases were really cases of anæmia, and that the essential intracranial change was clot formation in the cerebral veins and sinuses—a condition which, unless special search is made for it, may be readily overlooked even on post mortem examination.

Demanding more respect than either of the foregoing suggestions is the view advanced by Sir William Gowers—namely, that optic neuritis in chlorosis depends partly on continued congestion of the optic discs due to hypermetropia, and partly on a toxic or other abnormal quality of the blood. On this, with great deference, it may be remarked that hypermetropia is not present in all cases of chlorotic optic neuritis, and that, in any event, it is difficult to believe that eye-strain, even when acting in co-operation with some deleterious agent present in the blood, can explain the extreme degree of swelling of the optic discs which has repeatedly attracted attention in the cases now under consideration. Further, neither eye-strain nor toxæmia will readily account for a unilateral ocular paralysis, which, as has already been shown, occasionally accompanies the optic neuritis of anæmic women. Hence, in such a case, it is necessary, on the view now under discussion, either to invoke two separate and distinct causes—one for the optic neuritis and a second for the ocular paralysis—or to argue that optic neuritis accompanied by an ocular paralysis in a case of chlorosis owns a different cause from the one in operation when, in similar conditions, optic neuritis appears apart from such paralysis. It may be fairly said that neither of these alternatives offers an inviting choice. On the other hand, thrombosis, which, in the circumstances, must be admitted as at least a possibility, is adequate to explain all the facts—both the optic neuritis and ocular paralysis existing in association, and the optic neuritis present as an isolated event. In short, while any hypothesis other than intracranial thrombosis interposes a gap between cases of chlorosis accompanied merely by optic neuritis and cases of the same disease in which there exist together with optic neuritis various other evidences of cerebral disturbance, thrombosis brings all these cases into a common category, and, while recognizing a wide variety in the extent and severity of the clinical manifestations, includes all alike in an identical order.

Turning now from optic neuritis and ocular paralyses as occasional events in the clinical history of simple anæmia, attention may next be directed to certain disturbances of the visual fields which may exceptionally appear as complications in this disease. Probably these are very rare, and it is to the courtesy of my hospital colleagues that I am indebted for the opportunity of studying two instances which have come under my observation.

The first[1] was that of a woman, aged 34, in whom there was detected a right homonymous hemianopsia (fig. 14) unaccompanied by any other evidence of organic disease. This condition had been present for thirteen years, as was proved by perimeter charts in the possession of the patient and taken near the date of its onset. At that time she was, to use her own words, suffering from "dyspepsia and bloodlessness," and was being treated as an out-patient at a London hospital. Suddenly, one morning, when engaged in her occupation as a tailoress, she had an attack of "dizziness," and noticed that when she carried her hand to the right, as in the act of stitching, it disappeared from view; and that generally she was unable to see objects situated to her right side—a state of matters which still persists. Yet, with this exception, she has continued to enjoy fair health, and certainly has been, and still is, entirely free from all symptoms of active intracranial disease. The

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[1] Mr. W. Ilbert Hancock's case; reported in detail in the *Practitioner*, December, 1904.

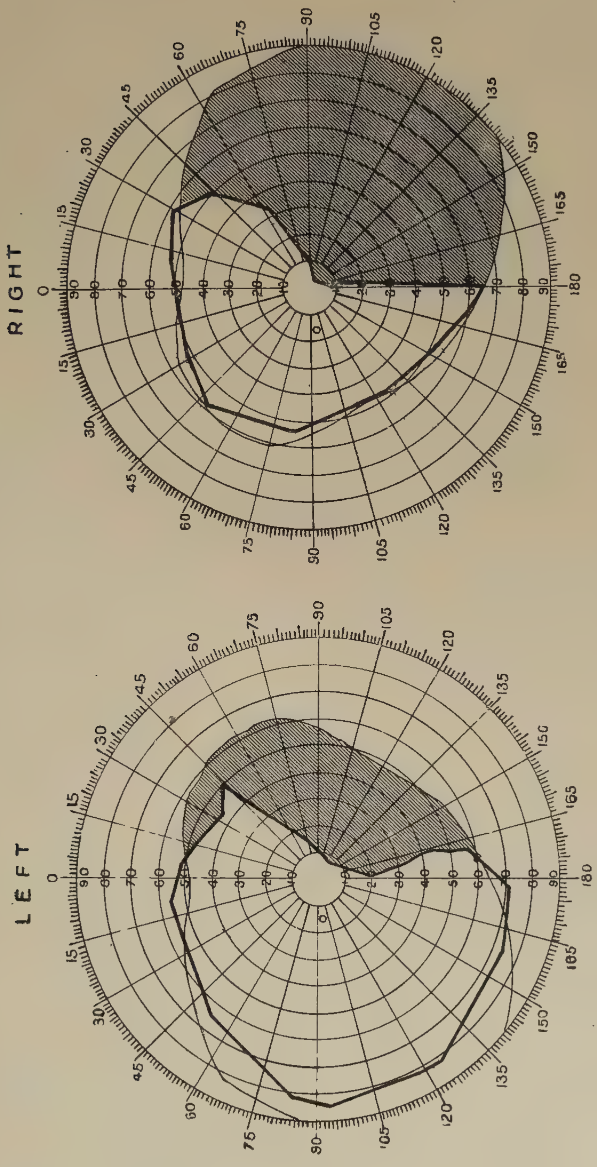

FIG. 14.—Right homonymous hemianopsia in a woman aged 34. Onset sudden when patient suffering from "dyspepsia and bloodlessness" at 19 years of age.

occurrence of the hemianopsia, it will be allowed, involves the existence of some cerebral lesion, and the sudden development of this symptom suggests most strongly that the lesion was of circulatory origin. The youth of the patient—21 years—other facts apart, may be taken as sufficient to exclude hæmorrhage. In these circumstances thrombosis inevitably suggests itself, and the relatively "quiet" character of the seizure favours such a view. Even if a diagnosis of embolism be preferred, the absence of cardiac valve disease makes it necessary to allow thrombus formation either in the heart itself or somewhere in the arterial system on the proximal side of the obstructed vessel. Hence, on either diagnosis, the case may be quoted to illustrate one aspect of the possible range of thrombosis in cases of anæmia. On the whole, the probabilities, including the presumption arising from the patient's age, may be claimed as favouring venous thrombosis, and if this is accepted, the case enforces a view already advanced—namely, that such thrombosis may cause a limited, though definite, clinical expression without at the same time producing anything like widespread evidences of cerebral disturbance.

A second case,[1] claimed to belong to the same order, is that of a woman, aged 32, who has been under observation during the last few years, and who, save for the facts now to be mentioned, is free from all evidence of organic disease. Her original complaint was of breathlessness and scanty menstruation, and she was found to be the subject of a moderately severe anæmia. Without any other disturbance she suddenly became aware that, as she put it, "I could not see round about me." The visual charts were taken, and a decided contraction of each visual field, but more particularly of the right field, was detected (fig. 15). Save this, there was no evidence of intracranial disease, and the general clinical examination of the patient, except for the fact of anæmia, was entirely negative. Gradually, in the following weeks, the fields moved towards their normal

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[1] Mr. T. Brittin Archer's case.

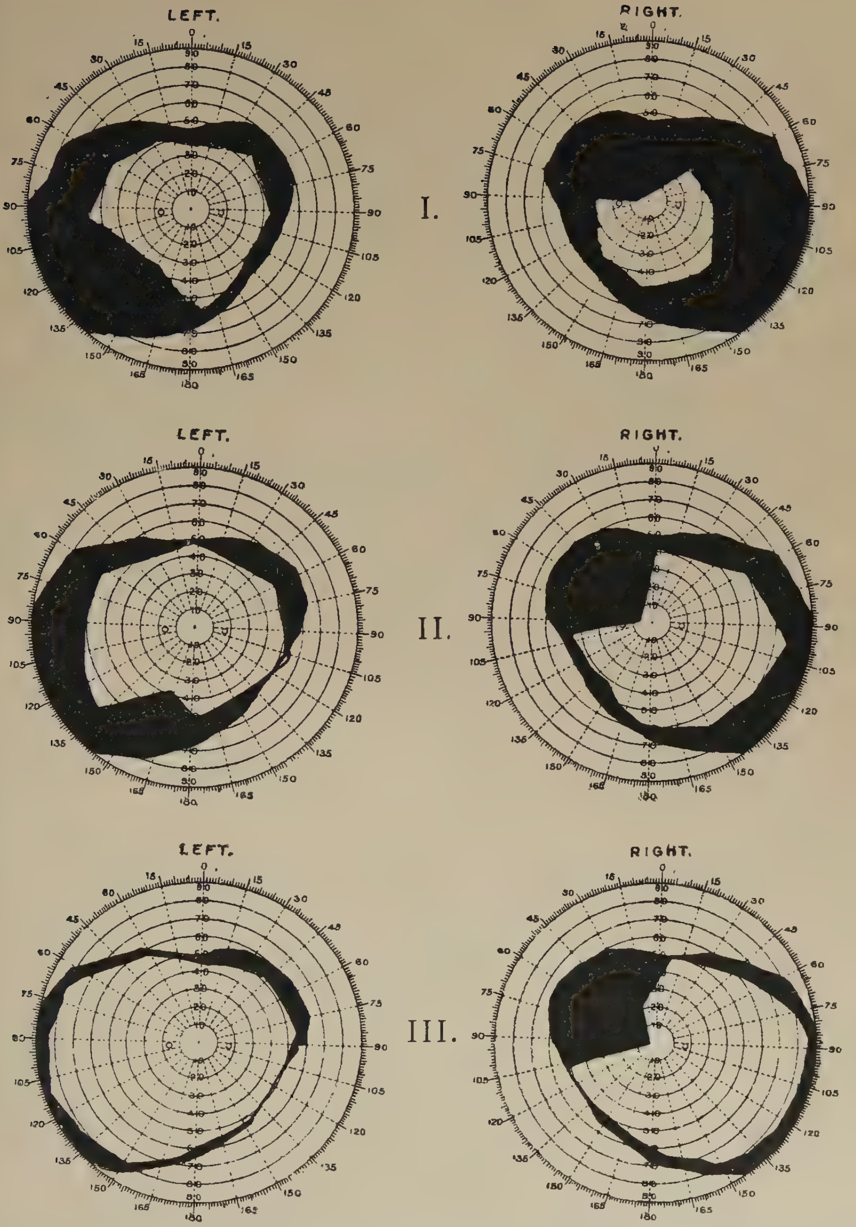

FIG. 15.—(I.) Charts taken in May, 1904. (II.) Charts taken in August, 1904. (III.) Charts taken in July, 1905, and confirmed on several later dates. From the case described in text, p. 130.

expansion, but even yet there is absence of peripheral vision in the upper nasal quadrant of the right field, and this, in view of the lapse of time, must probably be accepted as a permanent defect. Here, again, is a limited cerebral disturbance occurring suddenly in a woman free from all evidence of disease other than anæmia. In the circumstances, the suggestion of thrombosis in the higher reaches of the central visual nervous apparatus on the left side can hardly be resisted. Even if it is objected that the initial bilateral character of the visual disturbance necessitates a bilateral lesion, the argument in regard to thrombosis is not appreciably weakened. And further, it may be answered that a bilateral lesion as an explanation of the visual disturbances is by no means necessary. When it is remembered that in functional hemiplegia (see p. 292), where, presumably, the cerebral disturbance is confined to one hemisphere, contraction of the visual field, though most marked in the eye opposite to the inactive hemisphere, may be detected also, at least in some measure, in the eye of the same side as the "lesion," it is not difficult to accept an area of thrombosis, affecting the higher visual centres of the left brain, as the explanation both of a temporary limitation of the periphery of both visual fields and also of a permanent defect confined to the field of the right eye. This is the suggestion here offered as the interpretation of the present case, and the record is advanced as a further illustration of the range of intracranial thrombosis as this occurs in cases of anæmia.

I next submit retinal embolism as an event of occasional occurrence in anæmia and demanding thrombosis as its interpretation. In my own note-books I find records of three cases of embolism in which no abnormal fact other than anæmia could be detected. The patients were females, aged 21, 27, and 42 respectively. In none of the cases was there a history of rheumatism, and all alike were free from evidences of cardiac disease and of arterial degeneration. That the central artery of the retina was in each instance blocked there could be no doubt. For this, in the absence of cardiac valve disease, no obvious explanation could be found. Yet, from the existence either of embolism, or of thrombosis *in situ,* it was, in view of the fact of arterial obstruction, impossible to escape. Opinions may possibly differ as to which of these two is, in the circumstances, the more probable, but either involves the admission of clot formation in one or other part of the cardio-arterial system, and such clotting must apparently be attributed to the anæmia. It seems reasonable to suggest that the primary thrombosis took place in one or other of the two chambers of the left heart, and that the cases were therefore true examples of retinal embolism. In some of the fatal cases of chlorosis attended with intracranial venous thrombosis the presence of intracardiac thrombi has been observed, and thus the suggestion here proposed has a certain basis of actual anatomical observation in its favour. On the other hand, it is also true that primary arterial thrombosis has, in a few exceptional cases of anæmia, likewise been recorded, and therefore this alternative explanation of the cases here under discussion must be admitted as possible. But either view serves the present argument—namely, that obstruction of the retinal artery must be included among the complications which arise in anæmia as a result of the tendency in that condition to the formation of intravascular blood-clot.

Lastly, as an occasional complication in anæmia, and possibly due to thrombosis, may be mentioned retro-bulbar neuritis. This, in its typical form, is characterized by sudden loss of central vision in one eye, and without evident ophthalmoscopic change, although, at a later date, there may be whitening of the disc, due presumably to some measure of optic nerve atrophy. These facts demand a lesion in the trunk of the optic nerve posterior to the eyeball, and the suddenness of the event often suggests that the lesion is vascular in nature. It may, therefore, be thrombosis, and the possibility that complete recovery may occur is manifestly in entire harmony with this conclusion. No doubt retro-bulbar neuritis is also seen in conditions other than anæmia, but this does not qualify the experience that it is occasionally met with in patients free from all evidences of disease except anæmia (fig. 16). Nor does it negative the suggestion that the proximate cause of the condition, whether when associated with anæmia, or otherwise, may be thrombosis. It is no doubt true that retro-bulbar neuritis is sometimes an event in the evolution of disseminated sclerosis. But most certainly it exists apart from this disease. Indeed, it occurs in patients in whom clinical observation, even when extended over a term of years, fails to detect any evidence of organic disease; and some of these are anæmic women, that is, are patients in whom thrombosis is a well-recognized possibility.

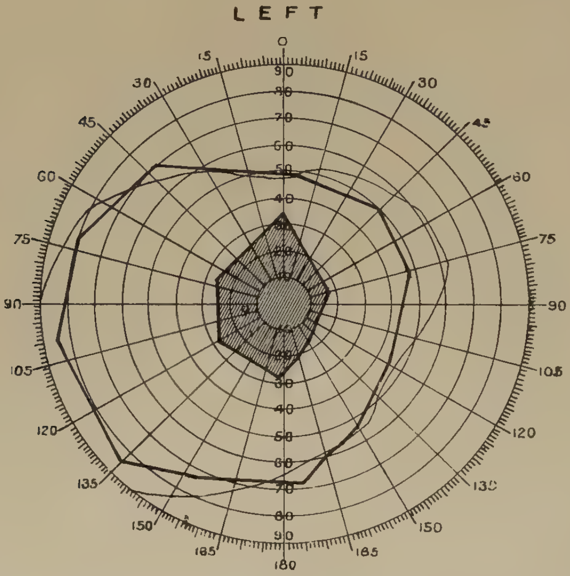

FIG. 16.—Central scotoma from retro-bulbar neuritis in a case of anæmia. Central vision was restored in about six weeks, and patient, when seen three years later, was free from all evidences of disease.

Thus there seem to be reasons to accept the view that while some ocular complications in anæmia, namely, optic neuritis and ocular palsies, may be due to thrombosis of the intracranial veins and sinuses, others may depend upon thrombosis in the vessels, probably the veins, of the peripheral visual apparatus, and among these latter may be placed retro-bulbar neuritis. But the possibility of a peripheral thrombosis cannot be limited to cases coming typically under this latter diagnosis. The fact is that the unilateral ocular complications of anæmia range from a greater or less degree of papillitis without prejudice to vision, to a condition of complete loss of visual acuity with little or no ophthalmoscopic change. And between these two extremes there are intermediate cases which show in various proportions, as it were, some measure both of papillitis and of visual defect. Assuming thrombosis to be responsible for these cases, it is readily conceivable that the several varieties and combinations may be determined by the particular site at which the formation of blood-clot takes place. Should this occur at or near the optic disc, it may well produce exudation and hæmorrhages of a more or less serious character upon and around the disc—a papillitis in short; and with such a condition, as is common knowledge, there is no necessary decline of visual acuity. On the other hand, should thrombosis affect vessels in the trunk of the optic nerve behind the eyeball, serious interference with vision, and especially with central vision, will be a prominent symptom, although, at least in the early stages of such a case, the fundus may appear normal or may show nothing more than some fulness of the retinal veins. In the intermediate cases, that is those in which papillitis and visual defect are both present, it may be surmised that thrombosis has involved vessels both on or near the optic disc and also those of the nerve trunk. One further remark in connection with these cases may be added—it does sometimes happen that a patient shows papillitis in each eye, and in one or both eyes, also, a serious reduction of visual acuity. The latter event, as already suggested, implies a lesion in the optic nerve trunk. Therefore, when, with double papillitis, there is marked visual loss, it seems probable that the thrombosis, assuming this to be the responsible lesion, is bilateral and peripheral, and further warrant for this view may be found in cases, described as retro-bulbar neuritis, in which the affection attacks both eyes, either simultaneously or in rapid succession.

There, therefore, may be distinguished in anæmia two groups of cases in which bilateral papillitis (optic neuritis) is present. In one there is marked visual defect, and here, for reasons just given, the probabilities are in favour of a bilateral peripheral lesion. In the other and more common variety, there is little or no impairment of vision. In these circumstances it is sufficient to assume, as has already been suggested, a central or intracranial thrombosis, and the experience that with this order of neuritis more or less conspicuous clinical evidences of cerebral disturbance are sometimes present may be advanced in support of such an assumption. But in both groups, as well as in other cases in which optic neuritis and visual loss, either one alone, or the two in combination, are present, it may be submitted that thrombosis affords a possible explanation of the facts, and that by such explanation these cases are readily brought into reasonable relations to other cerebral and ocular conditions which occasionally manifest themselves in patients, who, in spite of exact and continued clinical observation, fail to display anything abnormal other than a depreciated quality of the blood.

Such, then, in outline, are some of the cerebral and ocular complications[1] which may appear in the subjects of simple anæmia. It may perhaps be said that these accidents are very unusual. Still, they occur, and therefore demand explanation; and it is possible that in their slighter forms they would more frequently be discovered were examination made for them. Further, the infrequency of these complications is by no means out of harmony with the suggestion that they depend on thrombosis; as, however it may be explained, thrombosis, although a potential development in all cases of anæmia, only in rare instances takes shape and form as a definite clinical occurrence. If the view here advocated is accepted, the occasions of thrombosis are somewhat enlarged, but even so increased there is no desire to pretend that these occasions are either relatively or absolutely frequent. Whether few or many, however, it is to be desired that clinical events shall be duly arranged in their appropriate order and association. The hypothesis here advanced is an attempt in this direction. Assuming it to be justified, it brings under a common explanation a number of clinical experiences, which, apart from it, are isolated and detached alike from one another and from the state of anæmia to which, in some fashion or other, their existence is undoubtedly due. In the practical sphere, the suggestion that optic neuritis, ocular paralyses, hemianopsia or other abnormalities of the visual fields, retinal embolism, and retro-bulbar neuritis, occurring in anæmic patients, are probably due to thrombosis, is significant, because, on such interpretation, any one or more of these events appearing in an individual case must be accepted as the sign of a tendency to thrombosis in an unusually marked degree, and must therefore claim the prompt and appropriate therapeutic measures which such a conclusion demands.

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[1] Hæmorrhages in the retina are occasionally seen in patients suffering from chlorosis and may possibly be either direct or secondary to thrombosis. They are quite exceptional, and even when present probably not more than one or two will be detected. In pernicious anæmia, on the other hand, multiple retinal hæmorrhages are the rule

NOTES AND REFERENCES RELATIVE TO THROMBOSIS AND THE CEREBRAL AND OCULAR COMPLICATIONS OF ANÆMIA.

The original cases of optic neuritis in chlorosis recorded by Sir William Gowers, together with a statement of his views regarding the influence of hypermetropia, in association with the abnormal state of the blood, as a factor in the causation of the neuritis, are to be found in his "Medical Ophthalmoscopy," and in the *British Medical Journal*, 1881, Vol. i, p. 796. A paper by Saundby and Eales, in which the influence of hypermetropia in the same direction is discussed, appeared in the *Ophthalmic Review*, September, 1882; see also Saundby in *Birmingham Medical Review*, November, 1885.

Sir William Broadbent (*Transactions of the Ophthalmological Society*, 1880-81, p. 108) has reported two cases of young women in whom optic neuritis was associated with amenorrhœa; one of these was fatal, and "the only morbid appearance in the brain was effusion into the ventricles." Cases of double optic neuritis with a similar post mortem result are included in a paper by Williamson and Roberts (*Lancet*, 1900, Vol. i, p. 1350).

Examples of severe cerebral disturbance and optic neuritis in chlorosis, with intracranial thrombosis on necropsy, are recorded by Bristowe ("Clinical Lectures and Essays on Diseases of the Nervous System"), Sir Dyce Duckworth (*British Medical Journal*, 1896, Vol. i, p. 149), Lee Dickinson (*Clinical Society's Transactions*, 1896, Vol. xxix, p. 63), and A. Brayton Ball (*Transactions of the Association of American Physicians*, 1889, Vol. iv, p. 57). Cases with similar symptoms terminating in recovery have been reported by Bristowe (*Loc. cit.*), and by Buzzard (*British Medical Journal*, 1896, Vol. i, p. 150). Instances of fatal cerebral thrombosis without optic neuritis are noted by Sir R. Douglas Powell (*Lancet*, 1888, Vol. ii, p. 1124), and by Stephen Mackenzie (*Practitioner*, February, 1898); see also Brayton Ball's collected cases (*Loc. cit.*).

Professor Welch's article, in which the whole question of intracranial thrombosis in chlorosis is discussed, is in Sir Clifford Allbutt's "System of Medicine" (First Edition), Vol. vi, p. 200, and a list of published cases is given at p. 227. Sir Clifford Allbutt himself deals with the question in Vol. v, p. 508; and in Vol. vii, p. 574, James Taylor refers to the probability that some cases of optic neuritis originate in thrombosis in the cerebral veins or sinuses.

Instances of double optic neuritis with more or less cerebral disturbance in patients with chlorosis are recorded by Wardrop Griffith (*British Medical Journal*, June 9, 1888), Burton-Fanning (*Ibid.*, 1894, Vol. i, 1354), and Crawford Thomson (*Ibid.*, 1073), the last-mentioned including a case in which there was diplopia. One of Lee Dickinson's patients (*Loc. cit.*), in addition to optic neuritis and hemiplegia, had a "varying squint," and in another there was inequality of the pupils. In one of Griffith's cases (*Loc. cit.*) the patient was chlorotic and had extreme neuro-retinitis without any other evidence of intracranial lesion. While under treatment she fell on the ice and struck her head. Three weeks after this she became delirious, with febrile temperatures and dilated pupils. The necropsy showed a tumour of the occipital lobe, with some evidences of meningitis at the base of the brain—an illustration that chlorosis in association with double optic neuritis does not invariably exclude cerebral tumour from the diagnosis.

An instance of double optic neuritis and sixth nerve paralysis, practically identical with the one described on p. 116, is reported by Diabella (quoted in *Lancet*, September 19, 1896). Another is recorded by Sir William Gowers (*Transactions of the Ophthalmological Society*, 1889-81, p. 115); the symptoms in this instance included occipital pain and vomiting, and the diagnosis suggested was a transient localized meningitis. Several of the patients reported as examples of optic neuritis in chlorosis have also suffered from severe pain in the head and from vomiting. Burton-Fanning (*Loc. cit.*) relates such a case, and suggests that the absence of fever, etc., excludes basal meningitis from the diagnosis; he proposes increased intracranial pressure from œdema of the brain as the probable cause. It may be suggested that thrombosis is adequate to explain all the facts.

The rarity of sinus thrombosis as a condition producing severe symptoms in anæmia may be judged from the fact that Byrom Bramwell ("Anæmia and Diseases of the Ductless Glands") met with no example in an experience of 314 cases of chlorosis.

The references to cases of middle-ear disease with ocular symptoms are as follows: Newton Pitt (*British Medical Journal*, 1890, Vol. i, p. 648, *et seq.*); Marmaduke Sheild (*Archives of Otology*, 1892, p. 283); Whiting (*Ibid.*, 1898, No. 6); Clarence A. Veasey (*Ophthalmic Record*, June, 1899); Mayo Collier (*Transactions of the Laryngological and Otological Society of Great Britain*, July, 1901); J. Stoddart Barr (*British Medical Journal*, April 7, 1906); Adolph Bronner (*Ibid.*, October 12, 1907); Sir Wm. Macewen ("Pyogenic Diseases of the Brain and Spinal Cord"); Barr ("Manual of Diseases of the Ear"); the last-named mentions paralysis, either of the third or sixth nerve, as a result of thrombosis of the cavernous sinus.

Two important papers on the condition of the fundus oculi in suppurative disease of the middle-ear have been published by J. Stoddart Barr and Jno. Rowan. These authors, on a basis of 160 cases carefully observed, affirm that optic neuritis may occur in cases of purulent middle-ear disease free from obvious signs of any intracranial complications, and that in the same circumstances engorgement of the vessels of the fundus oculi, stopping short of neuritis, is frequent; they suggest that in serous meningitis (diffuse or localized) an explanation of these facts is to be found (*British Medical Journal*, November 23, 1907, and March 26, 1910). With these references may be associated instances in which middle-ear disease was complicated by an ocular paralysis without optic neuritis. Two cases of this character have been recorded by Professor F. de Lapersonne (*Medical Review*, September, 1901, p. 623).

A further reference of interest in connection with the argument advanced in the preceding address is an account of a case of enteric fever in which a unilateral third nerve paresis developed comparatively early in the disease, and the patient at a later date suffered from thrombosis of the right femoral vein. No other obvious cerebral disturbance accompanied the paresis, and lumbar puncture gave a clear fluid, free from organisms. The possibility of a limited meningitis must, of course, be accepted; but in the circumstances thrombosis can hardly be denied a hearing. The case is recorded by Chas. P. Emerson, in the *Johns Hopkins Hospital Reports*, 1900, Vol. viii, p. 897.

As bearing on the possible causation of optic neuritis by an intracranial blood-clot reference may be made to a case reported on p. 154 of the present volume.

ON RETINAL EMBOLISM WITH TWO CASES IN WHICH A NORMAL STANDARD OF CENTRAL VISUAL ACUITY WAS RETAINED.

Retinal embolism, as a clinical event, is signalled, in the great majority of cases, by the occurrence of sudden and complete blindness, and this either remains as a permanent condition, or undergoes, in course of time, some slight measure of favourable change.

When such sudden loss of sight has been preceded, on one or more occasions, by temporary visual disturbances or interruptions, the diagnosis of embolism comes more or less definitely under suspicion; and this applies, even although blocking of the central artery is made certain by ophthalmoscopic or other evidences. In such circumstances, thrombosis, endarteritis, and even arterial spasm, have been offered, either alone or in combination, as factors responsible for the vascular obstruction. The suggestion has even been made that it is in these causes the majority, if not indeed the whole, of the cases labelled in clinical descriptions "retinal embolism," must find their true explanation. There is, however, reason to suspect that this statement is set in terms too sweeping, as, apart altogether from clinical observations, there is fairly distinct pathological evidence that true embolism of the retinal artery does undoubtedly occur. Whether, in any individual case, this, or alternatively, some other cause of arterial obstruction, is in operation, must be decided by a study of the whole of the attendant circumstances. The question, as a pathological problem, is one of great interest, and its relations in this respect have been very fully discussed by Mr. George Coats[1] in a paper based upon a series of exact histological observations. As a clinical problem, however, the discussion can hardly be of more than academic value, for, given cessation of the blood-supply, however produced, the functions of the retina are likely to be permanently damaged, and this, even although the circulation should be restored after a comparatively brief interval.

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[1] *Royal London Ophthalmic Hospital Reports*, October, 1905.

In the first of the two cases here recorded (p. 148), the diagnosis of embolism can scarcely be questioned. The dramatic suddenness of the visual loss, the existence of mitral disease, and the ophthalmoscopic facts, taken together, hardly admit of any other interpretation. In the second case (p. 148) there may be reasons for a less confident conclusion. One of these is the absence, so far at least as the patient's judgment goes, of an abrupt or instantaneous character in the onset of the visual defect. Another is the non-existence of any evidence of cardiac or vascular disease from which an embolus could take origin. Yet the case is, beyond doubt, one of arterial occlusion. Hence embolism, thrombosis, and endarteritis, appear to be the only possibilities, for spasm *per se,* always a somewhat speculative suggestion, can hardly be proposed as an explanation of arterial obstruction sustained to such a point as permanently to destroy the function of the greater portion of the retina. Now of these three available causes, the comparative youth of the patient (28 years), the absence of any evidence of arterial disease, and the freedom of the personal history from any hint of syphilitic infection, render disease of the wall of the retinal artery in the highest degree improbable. There remain, therefore, thrombosis and embolism. Neither of these is without its difficulties. Against embolism is the absence of any manifest source from which an embolus could readily arise. Against thrombosis are all the circumstances which render endarteritis improbable, for arterial thrombosis is usually, if not invariably, secondary to disease of the vessel-wall. Even if the latter consideration be disregarded, it would have to be admitted that a thrombus, formed *in situ,* was a demonstration of an abnormal tendency to blood-coagulation. And if this could display itself in the retinal artery, it might equally well do so in some other part of the circulatory apparatus, say in one or other of the chambers of the left heart. The supposition of primary thrombosis may,

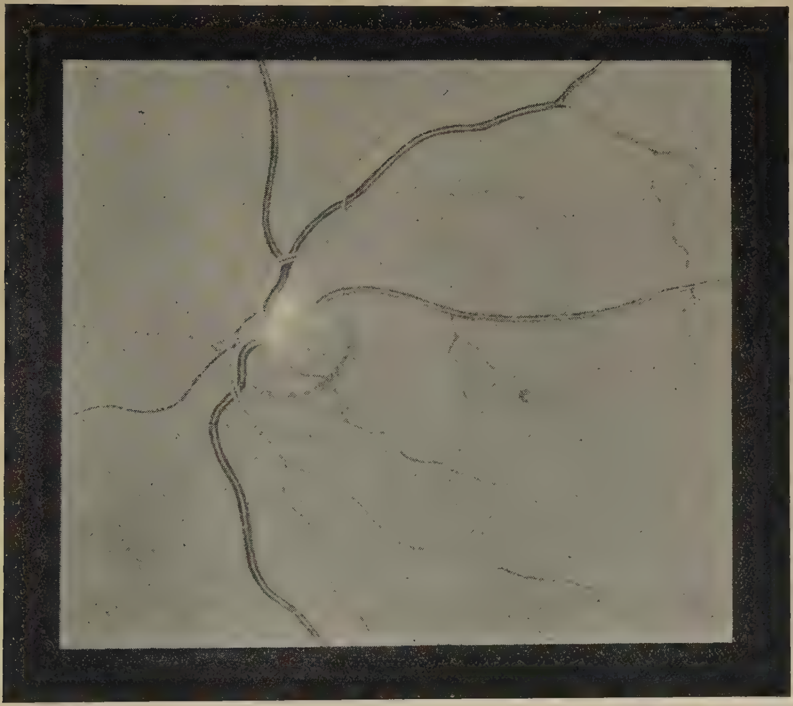

FIG. 17.—Fundus Oculi showing large Cilio-Retinal Artery supplying Macular Region, and Branches of Central Artery much reduced in calibre; some of the latter have "white lines" along their course, and one, with a downward and inward direction, is practically empty.

therefore, be made readily to apply either to an embolus or to a thrombus as the immediate cause of obstruction of the retinal artery in a patient free from cardiac and vascular disease. Granted this, it may be asked whether any cause for such primary thrombosis can be suggested. Now the examination of the patient reveals no sign of disease other than an anæmia of the chlorotic type. But, although perhaps not very frequent, thrombosis is a recognized complication of chlorosis. Occurring mostly in the veins, or in the intracranial venous sinuses, it does sometimes happen in the arteries. There can thus be little hesitation in admitting that it may also be possible in the chambers of the heart. But a thrombus formed in the heart may be detached, and, if swept from the left auricle or ventricle, may be carried, for example, into the central artery of the retina. Hence there is no reason why a block in this artery, occurring in association with anæmia, should not be due to embolism. In the absence of endarteritis, the block must mean either embolism or thrombosis. And, on the whole, it is perhaps easier to believe that anæmia, with a resulting measure of cardiac dilatation, should give rise to an intra-cardiac thrombosis, than that it should produce a localized arterial thrombosis apart from primary disease of the vessel-wall. Whichever of the two views just stated be accepted, it is manifest that among the thrombotic possibilities of anæmia, must be reckoned the occurrence of obstruction of the central artery of the retina, with the clinical consequences associated with the diagnosis of retinal embolism. In the individual case now under discussion (p. 148), the description of the onset of the visual loss, though far from conclusive, may be said to favour thrombosis *in situ* rather than embolism. This comment, however, does not touch, or at least is less appropriate to, some previous records,[1] and, in any event, it must be judged together with the general rebutting evidence already related. Hence the balance of evidence, though allowing some room for uncertainty, seems to favour the diagnosis of embolism, and it is certain that, for ordinary clinical classification, the present case exactly accommodates itself to this diagnosis.

Assuming, then, that each of the two cases here recorded is an example of embolism of the central artery of the retina, the most remarkable feature is the retention, in each instance, of a full measure of central visual acuity. Indeed, this peculiarity remains without diminution of interest, even if a thrombus or other agent is substituted for an embolus as an explanation of the arterial obstruction. The experience is only too general, that such obstruction carries with it complete loss of sight in the affected eye.

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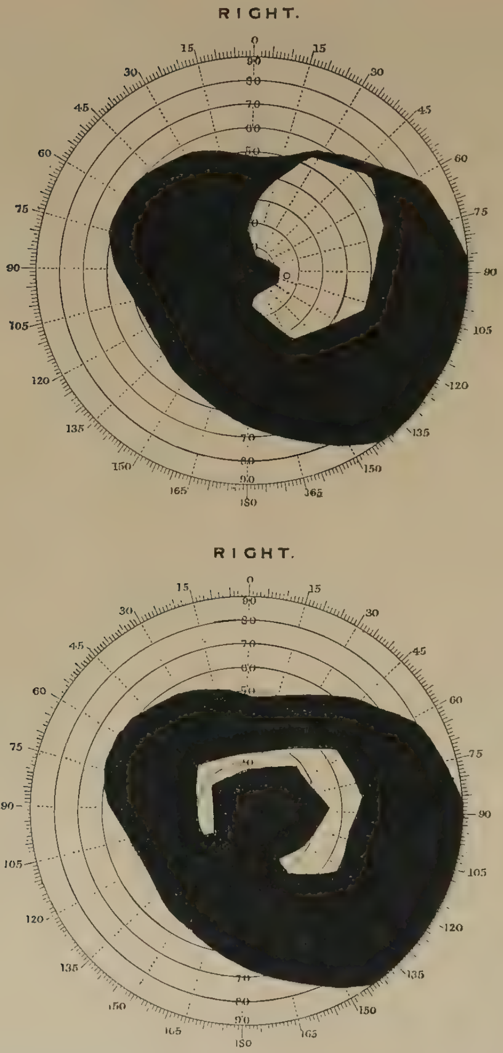
[1] *The Practitioner*, December, 1904.

Occasional cases of retinal embolism have been recorded in which, not the whole field, but some limited part of the periphery only has been lost, this being due, presumably, to obstruction, not of the main artery, but of one or more of its terminal branches. On the other hand, there have been instances in which, with retention of the greater part of the field, there has been a permanent central scotoma, as the result, apparently, of the entry of the obstructing clot into a macular artery, the other vessels of the retinal circulation remaining pervious; and this, as it involves loss of central vision, is almost as disastrous as obstruction of the main vessel. In the present cases each patient has perfect central or direct vision, but the field is restricted to a very limited area surrounding and including the fixation point. Manifestly, therefore, though the blood-supply has been cut off from the greater part of the retina, it has remained effective to the macular region.

It is, perhaps, not very uncommon to find, in cases of retinal embolism, that a small part of the field is retained, this being due to a supply of blood reaching, in one way or another, the corresponding part of the retina. The source of this supply may be either a branch from one of the ciliary arteries (cilio-retinal artery), or a branch which takes origin from the central artery of the retina in its course through the trunk of the optic nerve. As the obstruction, whether embolus or thrombus, usually blocks the main artery at the level of the optic papilla, the circulation in a branch, such as the one just described, is not affected, and, consequently, when such a vessel exists, the blood-supply, in spite of the block in the central artery, may be continued to a limited portion



FIGS. 18 and 19.—Visual fields from Cases of Retinal Embolism. The greater part of each field is obliterated, but vision is still possible in a limited area, which, however, is eccentric, and does not include the possibility of central vision. In taking the visual field of an eye deprived of central vision, it is not easy to secure fixation, and hence the definition of the field may be more or less inexact. But if the patient is instructed to place his index finger on the fixation point of the perimeter, and is told to look steadily at the finger, a confident field can, with a little patience, almost invariably be secured.

of the retina. Another possible explanation of the persistence of some part of the field of vision in cases of retinal embolism may be, as suggested by Coats (*Loc. cit.*), the establishment of capillary anastomoses between the ciliary and central visual systems. In figs. 18 and 19 are shown two perimeter tracings from cases of retinal embolism, and each exhibits a small eccentric visual field, the existence of which is to be explained in one or other of the fashions just described. Neither in these nor in other similar instances, as a rule, does the retained portion of the retina include the macular region, and there is, therefore, loss of central vision.

In each of the two cases here related, however, there was retention of central vision, and the explanation was evident on ophthalmoscopic examination. As is seen in fig. 17, and the two cases presented a practically identical picture), the blood-supply of the retina is here not wholly dependent on the central artery. Towards the outer part of the disc is another artery, which is at once recognized as a cilio-retinal vessel, a branch, that is, not of the central artery of the retina, but of the ciliary system. And, manifestly, while the branches of the central artery are greatly reduced in calibre, the blood-stream within them having been brought to a standstill and the distending force of the blood-pressure being therefore absent, the cilio-retinal vessel is of relatively large calibre, and is in the enjoyment of an active circulation. It is to the presence of this vessel, and to its distribution to the macular region, that retention of central vision is due, while, in consequence of obstruction of the central artery, the whole of the peripheral field is blotted out. The presence of a cilio-retinal artery is, of course, not very uncommon, but it is not always so well developed as here, nor so intimately related to the macular region. Unfortunate, therefore, as these two patients are as victims of retinal embolism, they are to be congratulated upon their escape from the most terrible consequences of this condition, namely, complete and permanent loss of sight in the affected eye. They retain, on the contrary, perfect sight in the most important part of the visual field, and for that happy circumstance, they are indebted to the arrangement which has provided each of the affected eyes with a special blood-supply to the macular region. This is, perhaps, the main interest of the two cases. But the second case has this further value, namely, that it emphasizes the possible range of the consequences of anæmia through the occurrence of intra-vascular blood-coagulation.

CASE-RECORDS IN ABSTRACT.[1]

CASE 1.—*Sudden loss of sight in left eye, return of full visual acuity, but permanent contraction of visual field; cilio-retinal artery in affected eye. Patient the subject of mitral disease.*

Wm. P., aged 24, was in his usual health when, in November, 1903, while at business, he suddenly felt dizzy and exclaimed: "I have gone blind in my left eye." Seen two days after this occurrence, vision was less than 6/60, and there was some haziness of the edges of the optic disc and an appearance of œdema over the whole of the central region of the fundus. The visual acuity gradually improved and ultimately reached 6/6, and the fundus assumed the appearances represented in fig. 17 the state of the visual field being shown in the corresponding chart (fig. 20). Patient was myopic to 2.0 dioptres, but with the corresponding spherical lens was able to read 6/6, and J1 at 12 in. Examination of the chest detected a systolic murmur over the cardiac apex, with accentuation of the second pulmonic sound; patient had had an attack of rheumatic fever in 1901. When seen in January, 1906, the facts showed no alteration in any respect.

CASE 2.—*Extreme contraction of left visual field with retention of acute central vision; optic disc pale, with slight evidence of neuro-retinitis; cilio-retinal artery*

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[1] For the opportunity of observing these cases, I am indebted to the courtesy of my colleagues, Mr. Britten Archer and Mr. Ernest Clarke.

present. Patient anæmic, but otherwise free from disease.

Rachael C., aged 28, suffered from whooping-cough at the end of 1903, and, in December of that year, noticed a sense of "stars shooting and brightness" before the left eye, with, after a few days, "dulness of sight" in this eye; this "dulness" has proved persistent. When seen in February, 1905, V.A. was 6/6 in each eye, the left field was reduced to a small area around the fixation point, and the ophthalmoscopic appearances were practically identical with those figured in the previous case (fig. 17). Beyond manifest anæmia, no evidence of disease could be discovered. The personal history was one of good health with some want of vigour during the last year or two. Patient the mother of three healthy children; one miscarriage.

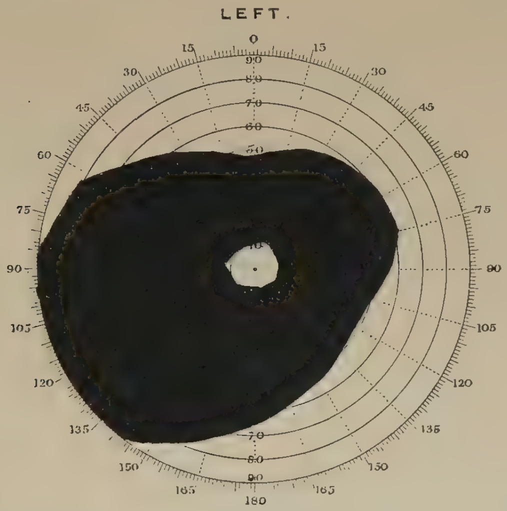

FIG. 20.—Visual field from Case 1; the fundus is represented in fig. 17. The field is obliterated except for a small area surrounding the fixation point where vision is 6/6 and J1.

General condition much improved under the administration of iron, but no alteration of visual field or ophthalmoscopic appearances to June, 1906.

ON THE CLINICAL ASPECTS OF METASTASES TO THE CENTRAL NERVOUS SYSTEM AND OTHER PARTS IN MALIGNANT DISEASE OF THE VISCERA.

In one of his articles on "The Medical Aspects of Carcinoma of the Breast," Professor Sir Wm. Osler states that "in many cases of internal metastases in cancer of the breast the symptoms are obscure or there are features which make the diagnosis doubtful." In illustration of this general proposition he quotes a number of case-records which show that the clinical difficulties created by such metastases arise from a variety of causes. In some cases the primary growth in the breast fails, on account of its small size, to attract attention, or it possesses features which conceal its true nature. In others, though the breast tumour has been recognized and removed, so long an interval has elapsed before the clinical evidences of internal metastases display themselves, that their real and sinister significance is apt to be overlooked. And in a third group, while one or other of the positions just defined may also exist, the symptoms caused by the metastases are so purely subjective that an interpretation of them as mere functional disturbances is readily adopted. This last-mentioned position seems to be reached in connection more particularly with the development of secondary malignant tumours in the spinal canal, and situated so as to involve either the spinal cord itself or the posterior roots of the spinal nerves. Pains in the back or sides, sometimes vague and ill-defined as to position, sometimes becoming of agonizing severity, "may persist for months," says Sir Wm. Osler, "without any sign of organic disease"; and even throughout the course of the illness—a period possibly of two years, or even longer—there may be neither paralysis nor sensory defect. In short, the diagnosis of metastases secondary to carcinoma of the breast may be missed, either from non-recognition of the original tumour, or from delay in the occurrence of the metastases, or from restriction of the clinical evidences to the subjective sensations of the patient.

It is now proposed briefly to describe some clinical experiences which show that what is true, as stated above, of carcinoma of the breast and its secondary developments, applies equally to carcinoma of the viscera and the metastases which may result therefrom. In one direction, at least so far as the non-recognition of the primary growth is concerned, this statement has no unfamiliar sound. Thus, it needs but a moderate acquaintance with the facts of clinical medicine to learn of cases of secondary cancer of the liver in which neither the history nor the physical examination gives any hint of the site, or indeed of the existence, of a primary malignant process. It happens that the clinical facts and associations of the secondary growths in such cases are often very conspicuous and free from ambiguity. Further, experiences of this order are relatively frequent events. Hence, here, not only are the results of the metastatic process usually recognized easily, but custom removes the surprise which might otherwise be expressed at the absence of all evidences of a primary tumour.

It is not unimportant to illustrate the proposition that an order of events not unlike that just described may also be noted when the primary malignant disease takes origin in organs other than the alimentary canal—such, for example, as the lung, pleura, or bladder—and where the secondary growths appear, not in the liver, but in the central nervous system, the bones, or other parts. Primary malignant disease of the lung or bladder, as is shown in cases recorded in this article, is, in its clinical aspects, by no means always an urgent and distinctive process, and the symptoms resulting therefrom may fail altogether to suggest the serious conditions on which they, as a matter of fact, depend. Hence, secondary or metastatic developments in such cases may readily fail to secure an accurate and ready interpretation, and if, as may be the case, the symptoms arising from such developments are entirely subjective in character, it is hardly surprising to find that they are apt to receive something less than clinical justice. The moral obviously is that with evidences of visceral disease of more or less obscure significance, and possibly, therefore, indicative of a malignant growth, subsequent disturbances, even though remote in time and situation and wanting in precision, should be regarded with active suspicion. Pain, particularly if severe, obstinate, or recurring, or referred to the back, or to the course of one or more of the spinal nerves, cannot, in such circumstances, be safely dismissed as hysterical or neurotic because of its freedom from association with objective signs of organic disease; the event may pass a painful commentary on such a conclusion. The only safe course is to suspend judgment, with a leaning, perhaps, to the more serious view, until either the appearance of conclusive evidence on the one hand, or the lapse of time without any such demonstration on the other, places the issue beyond dispute. Certain it is that objective evidence of the occurrence of metastases subsequent to primary malignant disease of the viscera, at least when the nervous system is the part secondarily involved, may not be obtained until very shortly before the patient's death, and it is conceivable that such evidence may even never appear.

In the first two of the following cases the primary malignant process, in one case in the lung, in the other in the bladder, gave rise to a mere minimum of local disturbance and to no admitted invasion of the general health and strength; and the patients were comparatively young women. There was thus in each instance little or nothing to excite suspicion of the true nature of the primary visceral disease. Hence, when nervous symptoms appeared as a result of metastatic extensions, the relation of these to the earlier symptoms of visceral disease was not easily suggested, and there was a temptation to suppose that the sequence was a purely accidental one. Further, the nervous symptoms were comparatively long in existence before objective demonstration of their association with organic change could be obtained. These two cases, therefore, illustrate the propositions which have already been stated. In a third case, whilst there was no doubt about the nature or the site of the original disease, the visual disturbance (hemianopsia) affords an interesting illustration of one among the many possible results of malignant metastases to the central nervous system. A fourth case is added to show how metastases to the bones may bear on the diagnosis of malignant disease of the viscera, the lung, in this particular instance, being the organ primarily attacked.

CASE 1.—*Malignant disease of the lung without active symptoms; later, severe pains in the lower limbs; evidence of intracranial disease a few days before death.*

The patient was a woman, aged 37 years, for whose earlier history I am indebted to Mr. Ernest W. Cross. With the exception of an ovarian cyst, which was removed by operation in 1902, the patient was free from serious illness until July, 1905; she then had an attack of pleurisy at the left base. The pyrexia and friction disappeared in the course of a few days, but some intrapulmonary crepitations were detected, and later there were physical signs of a moderate amount of pleural effusion. In November she was admitted to the Royal National Hospital for Consumption at Ventnor, where she improved generally, and the evidences of pleural effusion diminished. There was no expectoration and thus no search could be made for the bacillus of tubercle. Shortly prior to her admission to the hospital at Ventnor she had complained of pains in the lower extremities, and these became increasingly severe in the latter part of November, often then preventing sleep and demanding the use of morphine. When seen on December 18, 1905, the patient appeared to be a well-nourished woman, and her sole complaint was of pain in her lower limbs; the chest condition gave her no trouble. Nevertheless, there were signs of consolidation at the left base and some moist râles were present during inspiration. On physical examination no explanation of the pains could be detected, all the usual clinical tests of the condition of the nervous system giving normal results. On the 21st the patient was admitted to the Royal Waterloo Hospital, and in the course of a day or two it was observed that her speech had a slurred quality and that her mental condition was not clear. Soon she became noisy and semi-delirious and it was then found that double optic neuritis had developed. There were no other evidences of organic nervous disease and the temperature was normal or sub-febrile. Afterwards the patient became comatose and died on January 8, 1906.

The post mortem examination showed malignant disease (carcinoma) involving the lower third or so of the left lung, and some scattered malignant nodules in the liver. Of chief interest, perhaps, was the condition of the brain. Here, in the soft membranes over the convexity of the hemispheres, were several conspicuous hæmorrhages. The largest, having a diameter of about 1 in., was situated immediately behind the upper part of the right Rolandic fissure; superficially it had a pale jelly-like appearance and deeply it passed into the sulci and to a slight extent had torn its way into the brain tissue. A smaller hæmorrhage was present about the middle of the left parietal lobe and several others were scattered over the convolutions. With these exceptions the brain appeared to be normal; the spinal cord unfortunately could not be examined. There can, however, be no doubt that either the cord itself or the posterior nerve roots were directly disturbed by a secondary malignant process and that in this way are to be explained the severe pains which formed the prominent feature of the case. Hence it is of great interest to note that at no time, and in spite of repeated examinations, were there discovered any abnormal conditions of the tendon-jerks or other objective facts suggestive of organic disease of the cord. The pain was the one and only piece of clinical evidence of the existence of organic change of a malignant order in the lower part of the central nervous system. And it was not until a very few days before death that a demonstration of organic disease in any part of the nervous system could be obtained.

Of great interest, too, in relation to the actual intracranial lesion demonstrated at the necropsy, is the development of optic neuritis. There was no meningitis and no tumour, at least in the ordinary sense of the term. Yet it may be held that the blood-clot on the surface of the cortex was the physical equivalent of a tumour, and that it must bear responsibility as the cause of the neuritis. The only alternative, as an explanation of the neuritis, seems to be embolism of the central artery of each retina. Bilateral retinal embolism is not an easy diagnosis to accept, and, moreover, the ophthalmoscopic picture was the one familiar in optic neuritis and in no respect resembled that of embolism. Thus it seems certain that a blood-clot of moderate size and situated over the cerebral convolutions may cause optic neuritis, and presumably by the same mechanism, whatever that may be, as the one by which intracranial tumour establishes this result. Indeed, a blood-clot in such circumstances may come under the definition (Hughlings Jackson) of tumour as a "foreign body or adventitious product" within the skull. But if this is true of a clot outside the blood-vessels, it is conceivable that the same conclusion may apply to an intravascular clot—situated, say, in the cerebral veins or sinuses. Both in the one case and the other the bulk of the cranial contents is presumably increased. And as it has been suggested that the optic neuritis which occasionally occurs in chlorosis is to be explained as a result of sinus or venous thrombosis, the present case may be advanced as lending some support to this doctrine. Hence, in addition to the illustration which the case affords of the at times quiescent character of malignant disease of the lung, and of the clinical obscurity which often attends secondary malignant developments in the central nervous system, the record is not without bearing on the problem presented by the occurrence of optic neuritis in circumstances where there is reason to disbelieve in the existence of gross intracranial disease.

In the case next following, double optic neuritis is found in association with a malignant tumour situated in the cerebral cortex, and it will hardly be questioned that the relation between these two facts is one of cause and effect. The only difference between the two cases, in this respect, is, that in the one first related the " foreign body or adventitious product " in the cortex is a blood-clot, and not a neoplasm. To regard this clot as the cause of the optic neuritis, therefore, seems inevitable, and granted this, it is no large step to the proposition that a clot situated elsewhere within the skull—for example, in one or other of the intracranial sinuses—may in like fashion also produce optic neuritis (see p. 111 *et seq.*).

CASE 2.—*Metastases to brain secondary to carcinoma of the bladder; occasional hæmaturia the only symptom of the primary disease; headache of some months' duration and followed by ocular paralysis and double optic neuritis.*

The patient, a woman, aged 39, came to hospital complaining of pains in the head, " worse than an ordinary headache." The pains had troubled her for two or three months and recently had " caused " her to see double. The personal history was one of good general health, though in the course of the inquiry the patient stated that she had " for years " passed blood in the urine " every two months." Examination showed homonymous diplopia increasing to the right, though no manifest defect in the action of the right external rectus muscle could be appreciated; and the optic discs were regarded as " definitely suspicious." Otherwise there were no evidences of organic disease, and in particular the urine contained neither albumen nor blood.

The patient declined admission to the hospital but returned in a fortnight's time with obvious defect in the action of the external rectus of each eyeball and with marked double optic neuritis; the urine now contained blood, but no tube casts or other renal elements could be found. After her admission to the ward, headache and vomiting were troublesome symptoms, but beyond the ocular paralyses and the optic neuritis nothing further was discovered; and in the course of some ten days the patient became comatose and died. In view of the urgency of the cerebral symptoms no systematic attempt was made to examine the bladder, and the apparently isolated fact of hæmaturia, when taken together with the somewhat indefinite history of the bleeding contributed by the patient, caused this aspect of the case to receive less than its due share of attention. Yet at the post mortem examination it was found that the cause of the hæmaturia—namely, a malignant ulcer at the fundus of the bladder—was the starting-point of the whole train of subsequent symptoms. For, in the cortex of the parietal lobe of the left cerebral hemisphere was a considerable malignant tumour (carcinoma), and several small nodules of malignant growth were found scattered in the cortical substance of both hemispheres. There were no growths at the base of the brain, and the paralysis of both sixth nerves must have been due to the general increase of the intracranial pressure consequent on the growths in the upper part of the cerebrum—an illustration of the familiar truth that a sixth nerve paralysis has little or no localizing value. Some nodules of malignant tissue were present in each lung.

Here, as in the first case, the primary malignant disease caused little disturbance. Indeed, it was only when headache became troublesome that the patient thought it necessary to seek medical advice. Further, the headache had been in existence for two or three months before objective evidence of organic intracranial disease could be detected. And even then the connection between the hæmaturia on the one hand, and the cerebral symptoms on the other, was far from obvious. The case may thus be placed as a parallel to those instances of mammary cancer in which the primary tumour is not recognized, and in which, therefore, the interpretation of symptoms arising as a result of metastases to other organs is rendered difficult or impossible.

Case 3.—*Malignant disease of the lower bowel followed by right homonymous hemianopsia.*

In this instance there was no uncertainty in the diagnosis of the primary growth, as the patient, a man aged 60, had been operated on for malignant disease of the lower bowel. Shortly after the operation he complained that he could not see to his right side, and on taking his visual fields the charts as shown in the illustration (fig. 21) were obtained. In view of the right homonymous hemianopsia it seems certain that the patient was the victim of secondary malignant disease in the hinder part of the left visual tract, and the incompleteness of the visual defect suggests the left half-vision centre in the occipital lobe as the probable site of the secondary lesion. There was, at the time the charts were taken, no optic neuritis or other sign of cerebral disease. Though the case seems free from diagnostic difficulties it is not without value in a discussion on the clinical aspects of metastases to the nervous system and secondary to malignant growths in the viscera of the chest and abdomen.

Case 4.—*Malignant disease of lung; metastasis to femur.*

In the latter part of the paper already quoted Sir Wm. Osler refers to metastases to the bones secondary to cancer of the breast, and here again the sequence may find its parallel in cases of malignant visceral disease. In a patient recently under observation, a man, aged 62, the diagnosis of malignant disease at the base of the left lung came to be doubted, if not by the medical attendants, at least by the patient's friends, in consequence of the cessation of all active symptoms and of some improvement in the patient's general condition during a period extending over several months. Somewhat suddenly, however, as the patient was walking

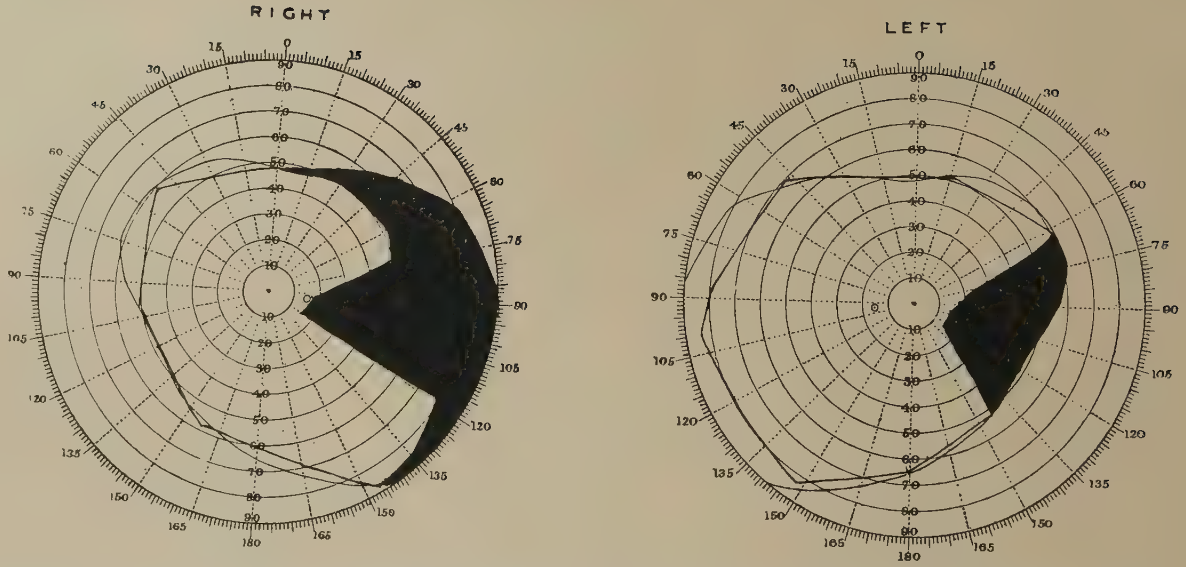

FIG. 21.—Charts showing incomplete right homonymous hemianopsia. The hemianopsia developed suddenly in a patient suffering from malignant disease of the lower bowel.

across his room, he became conscious of a severe pain in his left hip and of inability to use the limb. There was great tenderness over the neck of the femur but no actual tumour could be detected. The patient, however, never regained power over his limb, and, without much evidence of renewed activity in the diseased lung, gradually lost strength and died about three months after the above event. The absence of any appreciable tumour of the thigh cannot be regarded as sufficient to neutralize the presumption that the patient was the victim of a secondary malignant growth in the upper part of the femur, and that to this, the pain, tenderness, and helplessness of the limb must be directly credited. In this case the metastasis was an occurrence which, so far from confusing the diagnosis of malignant disease of the lung, strengthened this opinion at a time when the general condition of the patient appeared to bring the original view more or less definitely under suspicion.

REFERENCES.

In addition to Sir Wm. Osler's articles (*American Medicine*, April, 1901; *British Medical Journal*, January 6, 1906) and to the records quoted with these, interesting examples of malignant metastases to the brain and spinal cord have been reported by Spiller and Wisenburg (*Journal of Nervous and Mental Disorders*, August, 1906), and by Pinatelle and Cavaillon (quoted in Epitome, *British Medical Journal*, July 14, 1906). The following may also be consulted: "Tumour in Wall of Heart Secondary to Cancer of Œsophagus," Travers Smith (*British Medical Journal*, January 27, 1906); "Secondary Carcinomatous Growths Simulating Tuberculous Hip-joint Disease and Miliary Tuberculosis," R. E. Lord and W. Buckley (*Ibid.*, January 27, 1906); "Secondary Malignant Disease Limited to the Left Lobe of the Liver and the Suprarenal Capsules," C. O. Hawthorne (*Glasgow Medical Journal*, February, 1896); "Cancer of the Stomach with Extension to the Lymphatic System Through the Thoracic Duct," H. G. Turney (*Transactions of the Pathological Society of London*, Vol. xliv); Mitchell Stevens (*British Medical Journal*, April 20, 1905); and Nathan Raw (*Ibid.*, June 14, 1905).

ON THE SPHYGMOMANOMETER AND THE SPHYGMOGRAPH IN RELATION TO THE MEASUREMENT OF ARTERIAL BLOOD-PRESSURES.[1]

MR. PRESIDENT AND GENTLEMEN,—

IT is due, I cordially recognize, to your goodwill, that I enjoy the present opportunity of speaking to you on the relative values and meanings of two comparatively modern methods of observing and recording certain features of the arterial pulse; and although in doing so, I shall have, I fear, to recite a number of propositions with which you are already abundantly familiar, I hope to be able to convince you that some of these demand interpretations differing, in certain respects, from those which are commonly placed to their credit.

The arterial pulse has, of course, always been an object of interest and of importance to practitioners of the art of medicine, and the several varieties of its rate, of its rhythm, and of the characters of its individual beats, were recognized and expounded at a very early date in medical history. I am not sure whether anyone at the present day is prepared confidently to identify each one of the seven-and-twenty forms of pulse described by Galen, but I am quite sure that no modern physician proposes to enlarge that classification; and a good deal might be said for the contention that in practical clinical work the several characters of the pulse mean to-day very much what they meant to the physicians who first described them many centuries ago. But the modern spirit, inspired by science, with its universal passion for measurement, has touched the pulse as it has touched many other commonplace objects and movements. Hence has arisen the desire, in the study and examination of the pulse, to substitute, for the immediate sense perceptions and personal judgments of the physician, methods carrying a capacity for numerical values and graphic record—methods, that is to say, which may be relied on to yield results unaffected by the personal bias or other defects of the individual observer. The ambition, in short, of instrumental methods in the study of the pulse is to describe the pulse in terms of exact measurement and to display its characters in graphic or pictorial record.

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[1] An Address delivered to the Glasgow Southern Medical Society.

It may, perhaps, be said that this is a subject of but limited interest, seeing that the conditions under which medical practice is for the most part conducted are not favourable to the use of instruments of precision in ordinary pulse examinations. Such a proposition might easily be contested, but allowing it to pass for the moment, it still remains true that instrumental methods *are* used in the examination of the pulse, and that records so obtained, and important clinical inferences based on them, occupy a somewhat prominent position in modern medical literature. Consequently, whether we like it or not, we are, individually, face to face with the necessity of endeavouring to form an opinion of the value these records possess and of the meanings they severally convey.

Now it will be allowed that one quality of the arterial pulse which in modern medicine is being specially subjected to instrumental investigation and record, is the quality known as blood-pressure. And manifestly, before we can test or criticize these investigations and records, we must be familiar with the facts which they portray, and we must be ready, one and all, to accept and understand the term " blood-pressure " in one and the same sense. We must, in short, obey the elementary mandate which directs us to precede discussion by a definition of the terms to be employed.

Fortunately there is one part of the circulation—

namely, that conducted in the capillaries—where there is no room for difference of opinion or of statement in the definition of the term "blood-pressure." Through the capillaries, as we know, the blood flows in a steady and even current and at a constant and uniform pressure—at a pressure sufficient to move the blood along the vessels, sufficient to keep the capillaries distended, sufficient to cause certain elements of the blood to pass through the capillary walls, but, happily, at a pressure less than sufficient to produce capillary rupture. And this intracapillary blood-pressure is the essential blood-pressure of the body, as upon it, the maintenance of the nutritive and metabolic activities of the tissues directly depends; and all other blood-pressures, whatever they be, exist with this one aim and object—namely, to provide and continue a constant, uniform, and gentle blood-pressure in the capillaries.

But to institute and maintain a flow of fluid, that is a circulation, in a system of closed tubes, it is necessary to secure at some point in the system a rhythmical disturbance of pressures. And this, we recognize, is effected in the human body by the action of the heart, each systole of which causes a rise of pressure within its cavities, the resulting outflow of blood being guided in the desired direction by means of the cardiac valves; while at the following diastole there is a fall of the intracardiac pressure, and a consequent flow of blood into the cardiac chambers. Thus intracardiac blood-pressure is not, as is intracapillary blood-pressure, a constant and uniform level, but is, on the contrary, a repeated and rapid movement between wide extremes. Hence arises a necessity for the provision in the circulatory apparatus of some mechanism by which the wide variations of blood-pressure due to the alternate systole and diastole of the heart, shall be converted into the uniform and gentle pressure necessary to, and characteristic of, the capillary circulation.

It is to meet this demand that the arteries or tubes which intervene between the heart and the capillaries are provided with highly elastic walls. Into these elastic vessels at each systole of the left ventricle there is driven forcibly a parcel of blood, and while part of the force is occupied in moving the blood along the vessels, part is spent in distending or stretching the arterial walls. At the cessation of the ventricular systole these elastic walls recoil, and thus cause a further displacement of blood towards and through the capillaries. There is, therefore, in the capillaries, pressure and blood-flow during cardiac systole, and pressure and blood-flow in the intervals between successive cardiac systoles, this latter result being due to the elastic recoil of the arteries. In this way there is secured that uniform and constant intracapillary blood-pressure which the physical and biological conditions of the capillary circulation demand; while, at the same time, the anatomical and physiological distance of the capillaries from the heart, the resistance offered to the circulation by the arterial walls, and the elastic, and therefore force-absorbing, quality of these walls, combine to reduce the violent pressures caused by the cardiac contractions to a level compatible with capillary integrity.

But observe the unfortunate position of the arteries in this scheme of things, for their lot is indeed a hard one. With each cardiac systole a blow and strain is inflicted on the arterial wall. Recovering from this, the artery recoils, or moves towards a position of rest; but such a position it never reaches, for before it does so it is made to stagger under the impact of a second blow. Thus, always, the arterial wall is in a state of strain or tension—the tension rises with the cardiac systole and falls with the cardiac diastole, but it never wholly disappears; which is only another way of saying that always within the arteries there is a positive blood-pressure—the pressure rises with the cardiac systole and falls with the cardiac diastole, but it never wholly disappears. And the arteries suffer this strain, and these repeated bangings and buffetings, not for their own pleasure and enjoyment, but in order that the capillary circulation may be conducted in calm and quietude and peace—in order that the tissues, so far at least as the capillary circulation is concerned, may dwell at ease in a land " in which it seemeth always afternoon." Or, to change the figure, it is the fate of the arteries to "wither," in order that the capillaries may be "more and more."

In any discussion on the subject of blood-pressure it is only seemly and just that at least a passing word of recognition should be offered to the position, the function, and the fate of the arteries. Inelegant, unæsthetic structures, mere pipes or conduits for the transmission of fluid, the arteries may readily, at least by a superficial observer, be numbered among what the Apostle Paul calls "our uncomely parts," but when one recognizes how persistently they suffer "the slings and arrows of outrageous fortune," and how uncomplainingly they discharge their altruistic functions, we may surely go further and join with the Apostle in his conclusion :—

> "And those members of the body which we think to be less honourable, upon these we bestow more abundant honour; and our uncomely parts have more abundant comeliness."

This, however, is but a passing allusion to what may be called the poetical or philosophical relations of blood-pressure, and with these aspects of the subject, I recognize, we have at the moment no direct concern.

To revert, therefore, to a more practical note, it appears that while in the capillaries blood-pressure is a uniform and constant measure, and in the heart a series of rapidly-repeated excursions between a very high level and a very low, indeed a minus, quantity, blood-pressure in the arteries is a continuous force, though the amount or degree of this force rises and falls with the cardiac systole and diastole respectively, and the height of the maximum and the extent of the oscillation are greater as the artery is nearer the heart, upon the action of which both maximum of pressure and extent of oscillation directly depend.

By this brief sketch, then, we may be reminded that the pressure of the blood in different parts of the circulatory mechanism not only varies in degree but also in quality or kind—there is an intracardiac blood-pressure, an arterial blood-pressure, a capillary blood-pressure, and a venous blood-pressure; and each of these has both its own measure and its own character. Rather, therefore, than *a* blood-pressure there are blood-pressures; and hence, when a particular figure is presented to us as a measure or index of blood-pressure, it is of the greatest importance in the interests of clear thinking that we shall have a lively appreciation both of the point or level of the circulatory mechanism at which such measurement has been taken, and also of the exact period of the circulatory rhythm during which it has been calculated.

How, now, do these considerations apply to the use of the apparatus, the sphygmomanometer, which is being largely employed at the present day to estimate what is called, usually without phrase and without qualification, *the* blood-pressure? In the most common type of sphygmomanometer a broad leather armlet is strapped firmly round the upper arm; lining this armlet is an elastic bag, which communicates through an indiarubber tube with the well of a mercurial manometer, so that at any given moment the pressure at all points of the system is uniform, and the degree of the pressure is indicated by the height to which the mercury rises on the manometer scale. After the armlet has been fixed in position the pressure in the manometer system is gradually increased by pumping in air by means of a valved ball or pump. This increase is pushed until the radial pulse just disappears; and at this point the level of the mercury is recorded as the index of the blood-pressure, the assumption being that when the pressure in the sphygmomanometer is sufficient exactly to neutralize the pulsations of the brachial artery (as shown by the cessation of the pulse at the wrist), the pressure on the external surface of the artery must be equal to the pressure on the internal surface; and as this external pressure is indicated by the level of the mercury, the figure which this reaches is announced as the index or measure of the blood-pressure. Perhaps it is allowed, more or less generally, that considered as an absolute record this figure is qualified to an uncertain extent by the resistance of the soft tissues of the upper arm and also by the resistance offered by the arterial wall. But with this aspect of the question we are not now concerned. In other words, I propose to accept in the meantime, and without any discussion of the disturbing elements just mentioned, the figure recorded by the sphygmomanometer and usually announced as the measure of the blood-pressure, and upon this announcement to submit one or two comments.

First, it is evident, that the figure obtained from the sphygmomanometer is not the measure of anything which can be called the general, mean, or average blood-pressure, but is, on the contrary, merely the measure of a particular or individual pressure obtained from the brachial artery during a certain phase of the circulatory movement. Secondly, it is also evident, that the figure recorded by the sphygmomanometer is not the measure of anything which can be called the mean or average pressure of the arterial cycle, for the record is taken only at a single point in this cycle. And as the point selected is the one at which the distending force within the artery is just overcome, it is manifest that the sphygmomanometric figure is the measure of this distending force. But the distending force within the brachial artery is most certainly the pressure of the blood in so far as this is due to the systole of the left ventricle. Therefore the figure yielded by the sphygmomanometer is a measure of the force of the left ventricle as this reaches and influences the brachial artery; or, in more detail, it is a measure of the vigour with which the left ventricle, through the column of the blood and for an undefined period of time, acts as a distending or expanding force within the brachial artery.

It is of some importance carefully to note this definition, as the pressure of blood within the brachial (or any other) artery, is, if the complete arterial cycle is contemplated, caused, or at least controlled, by two different factors; and while one of these is the systole of the left ventricle, the other is the degree of resistance offered to the circulation as this resistance is varied by a contracted or relaxed condition of the vessels on the remote or peripheral side of the artery. But as, in the application of the sphygmomanometer, and at the

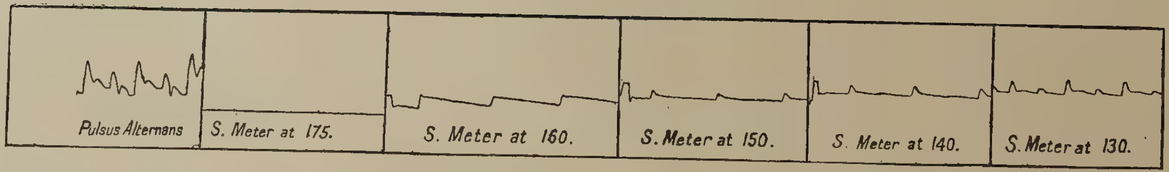

Fig. 22.— Sphygmogram (pulsus alternans) showing the effect on the radial pulse of different degrees of pressure applied to the brachial artery by the sphygmomanometer. With the pressure at 175 mm. Hg. all pulsations cease; at 160 and 150 only the alternate and stronger cardiac systoles reach the radial artery; and it is not until the pressure is reduced to 140 that the alternate weaker beats affect the tracing.

moment the record is taken, the peripheral circulation in the upper limb, is, by the strangling action of the sphygmomanometer armlet, and for the time being, practically put out of existence, it is obvious that the sphygmomanometer cannot measure the contribution which this factor—that is, the peripheral resistance—makes to the level or other character of the blood-pressure. There remains, therefore, for measurement merely the contribution made by the left ventricle, and hence we arrive again at the conclusion that the figure announced by the sphygmomanometer is, and only is, a measurement of the height to which, for an undefined period of time, the blood-pressure rises under the stroke or systole of the left ventricle.

I have here a record which I submit as a demonstration of the accuracy of this conclusion. The patient from whom this tracing (fig. 22) is taken has, in the language of the sphygmomanometer, a blood-pressure of 175; this, in other words, is the figure at which the mercury stands when the pressure in the sphygmomanometer is just sufficient to neutralize the arterial pulsation in the upper limb. When a pulse tracing (fig. 22) is taken it is seen that the pulse has the characteristic features of the pulsus alternans, a strong and a relatively weak beat following one another alternately. If now the sphygmomanometer and the sphygmograph are applied at the same time, and the pressure in the former is raised so that the mercury touches the level 175 on the scale, the excursion of the sphymographic needle ceases. But if the pressure is lowered, say to 160, certain beats of the pulse come through, and it is obvious that these are the alternate stronger beats; the same thing is seen when the pressure is further lowered, say to 150, and it is not until the pressure has fallen to 140 that faint indications of the alternate weaker beats of the heart reach the radial artery and appear as feeble waves in the pulse tracing.

According, therefore, to the doctrine of the sphygmomanometer, this patient at one moment has a blood-pressure of 175 and at the immediately following moment a blood-pressure of about 145. But the conditions attending the two observations have remained unchanged with one exception, and that exception is the degree of force of the cardiac systole. It is therefore a difference in the vigour of the alternating systoles which explains the difference in the two sphygmomanometric readings. Hence the facts displayed in fig. 22 may be quoted in support of the conclusion that what the sphygmomanometer measures is the force of the cardiac systole as this arrives at the brachial artery. Such, then, and subject to the qualifications to which I have already alluded, is the contribution offered by the sphygmomanometer to the measurement of the pressure of the blood within the arterial channels.

We have now to consider what information relative to blood-pressure is professed or offered by the sphygmograph. With the use of this instrument as an apparatus for recording the rate of the pulse, the rhythm of the pulse, and the presence or absence of pulse irregularities, we are not now concerned. Our business is solely with the characters of the individual sphygmographic wave in so far as these offer, or profess to offer, information as to the height or level of the blood-pressure. It may, perhaps, be said that no one uses, or thinks of using, the sphygmograph for the purpose of measuring blood-pressure. But, with great respect, I submit that this statement can only be accepted subject to considerable qualification. No doubt it is true that no one uses the sphygmograph with a view to make an absolute measurement of blood-pressure—a measurement, that is, in exact numerical terms. But surely it is also true that we are constantly presented with sphygmograms which are announced as indicating high blood-pressure on the one hand or low blood-pressure on the other. And the terms "high" and "low" in such a context are manifestly terms of measurement—of comparative, not of absolute, measurement—but, all the same, terms of measurement. Therefore it is fair to say that the sphygmograph is claimed as an instrument which possesses the ability to proclaim, at least in comparative terms, the height or level of the blood-pressure; and, indeed, diagrams such as those seen in fig. 23 are, with their corresponding descriptions, familiar objects in every text-book.

What, then, are the features of the sphygmographic wave which present themselves as guides in the endeavour to estimate the degree of blood-pressure? These characters are found, I venture to remind you, mainly on the descending line of the wave. When this line is sloping or oblique, with a second or tidal wave, at, or nearly at, the same horizontal level as the primary or percussion wave, and with a third or dicrotic wave relatively inconspicuous and high up on the descending stroke, the message of the sphygmograph is read as a message of "high" blood-pressure (fig. 23, *A*); while,

FIG. 23.

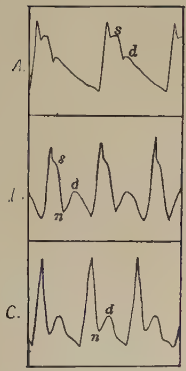

FIG. 24.

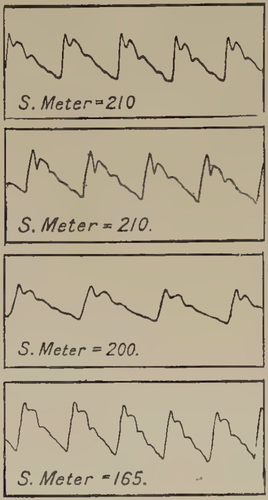

FIG. 25.

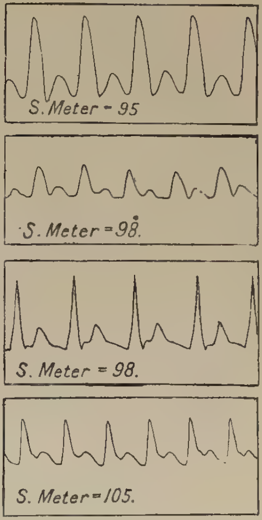

FIG. 23.—*A*, Pulse of high arterial pressure. *B*, Pulse of low arterial pressure showing slight tidal wave. *C*, Pulse of low arterial pressure without tidal wave. *s*, Tidal wave; *d*, dicrotic wave; *n*, pre-dicrotic or aortic notch. (Copied from Dr. Jas. Mackenzie's book on "The Pulse.")

FIG. 24. -Sphygmograms indicative of "high" arterial pressure associated in each case with a "high" sphygmomanometer record.

FIG. 25.—Sphygmograms indicative of "low" arterial pressure associated in each case with a "low" sphygmomanometer record.

on the contrary, when the wave falls suddenly and promptly, with a tidal wave either absent (fig. 23, *C*), or, if present, well below the horizontal level of the primary wave (fig. 23, *B*), and a dicrotic wave well marked and low down on the descending line, the sphygmographic reading is recorded as one of "low" blood-pressure. These are the facts upon which we are taught to rely in an endeavour, by means of the sphygmograph, to determine whether the blood-pressure or arterial tension in any individual patient is "high" on the one hand or "low" on the other.

Such, then, are the current doctrines of the sphygmomanometer and the sphygmograph relative to the measurement of blood-pressure, and it is manifest, if these doctrines are valid, that the two instruments in any given instance will deliver an identical message—they will speak in equal tones and will prophesy with a unanimous voice. Or, in practical phrase, when in any patient the sphygmomanometer announces a high record (say 130 or upwards), the sphygmograph will produce a picture of the type seen in fig. 23, *A*; while when the sphygmomanometer intimates a low blood-pressure (say 100 or less), the sphygmographic tracing will follow the pattern of fig. 23, *B* or *C*. And there are many cases in which this anticipated harmony or correspondence between the deliverances of the sphygmomanometer and those of the sphygmograph is fully and exactly realized. Thus in fig. 24 is shown a group of pulse-tracings, each one of which, in the language of the sphygmograph, is a proclamation of "high" blood-pressure—the descending line of the sphygmographic wave is oblique or sloping, the tidal wave is at, or nearly at, the same horizontal level as the percussion wave, and the dicrotic wave is slight and is high up on the descending line. And with equal distinctness the sphygmomanometer, in each case, also announces that the blood-pressure is "high"; in the first case the record is 210, in the second 210, in the third 200, and in the fourth 165, all of these numbers being far above the normal or average sphygmomanometric level. Again, in fig. 25

is seen a group of pulse-tracings every one of which, described in sphygmographic terms, is a distinct intimation that the blood-pressure is "low"—the pulse wave falls more or less suddenly, the tidal wave is absent, and the dicrotic wave is relatively conspicuous and is low down on the descending line. And the

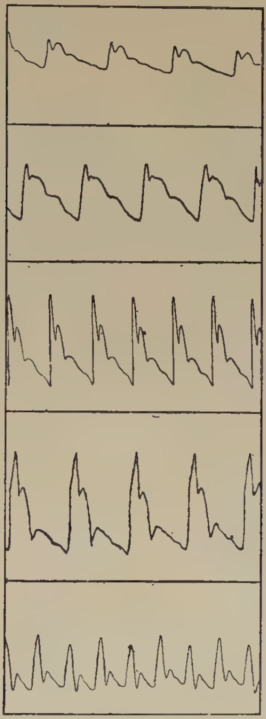

FIG. 26.—Sphygmograms of different and even of opposite character, with, in all, one and the same sphygmomanometer record—viz., 175 mm. Hg.

sphygmomanometer, in each instance, again speaks to the same effect, for the figures 95, 98, 98 and 105, as marked on the tracings, are as sphygmomanometric records distinctly below the usual or average reading.

Therefore, so long as we confine our observations to such series of cases as are represented in figs. 24 and 25, we may entertain and cherish the comfortable conviction that the sphygmomanometer and the sphygmograph in their deliverances on the subject of blood-pressure confirm and support one another—that they announce similar or corresponding findings, the one in terms of exact measurement, the other in the form of a pictorial display on which a comparative estimate may be securely rested.

But I have now to ask your attention to some records which seriously disturb this apparent agreement between the sphygmomanometer and the sphygmograph, which, indeed, completely and absolutely explode and destroy it. The first evidence to this end I show in fig. 26. Here is a group of pulse-tracings taken from different patients and presenting widely different characters. Especially do they differ among themselves in those features which are presented as pictorial guides to the estimation of the level of the blood-pressure. In the earlier members of the series it is written in unmistakable characters that the blood-pressure is "high," while in the later tracings the facts, with equal distinctness, announce that the blood-pressure is "low." Yet, and this is the startling fact, the sphygmomanometer proclaims that in each of these cases the blood-pressure has one and the same level—namely, 175 mm. of mercury. According to the one instrument, therefore, all of these patients have one and the same blood-pressure; according to the other, the blood-pressures in the several cases are widely different.

Clearly, then, we can no longer say that the sphygmomanometer and the sphygmograph speak in equal tones and deliver an identical message, for, with one and the same announcement from the sphygmomanometer, there may exist, as we here see, sphygmographic pictures which, as displays of the level of the blood-pressure, vary in their characters and meanings between the widest possible extremes. But this is not all. Not only with a uniform sphygmomanometric reading may the pulse pictures vary as just described, but the records of the two instruments may appear in direct and mutual antagonism. And the contradiction may be, as one may say, in both directions. In a given case, for example, the sphygmomanometer may intimate a "high" blood-pressure, while the sphygmograph may protest that the pressure is "low"; or, on the other hand, the sphygmomanometer may indicate that the pressure is "low," while the sphygmograph writes distinctly that the pressure is "high." Here is the demonstration. Fig. 27

FIG. 27.

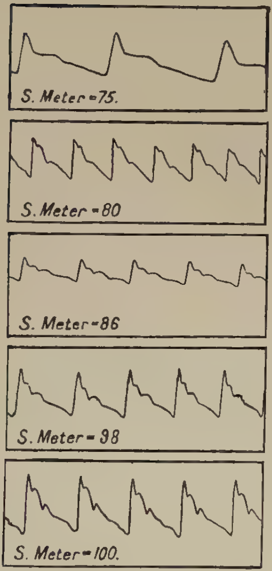

FIG. 28.

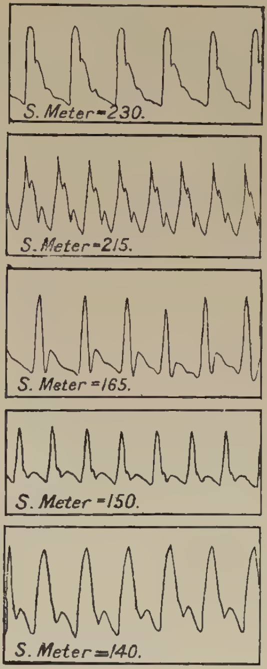

FIG. 27.—Sphygmograms indicative of "high" blood-pressure associated in each case with a "low" sphygmomanometer record.

FIG. 28.—Sphygmograms indicative of "low" blood-pressure associated in each case with a "high" sphygmomanometer record.

shows a group of pulse-tracings which, in sphygmographic language, announce more or less distinctly the existence of "high" blood-pressure. Yet, in every instance, the measurement effected by the sphygmomanometer is a measurement of "low" pressure, the several numbers running 75, 80, 86, 98, 100; and every one of these being distinctly below anything which can be called the normal or average sphygmomanometric level.

Again, in fig. 28 is another group of pulse-tracings, each one of which translated into sphygmographic terms carries a message of "low" blood-pressure. But the sphygmomanometer tells an entirely different story. It says, indeed, that in each of these patients the blood-pressure is above the normal, and in most of them, indeed, far above the normal, the actual numbers ranging from 230 to 140, while an average sphygmomanometric reading is somewhere about 120. In both of these groups, therefore, the sphygmomanometer and the sphygmograph speak, not with corresponding but with contradictory voices, and the anticipated harmony has failed to arrive. Hence, to present the position in a sentence, we find that while in certain instances the sphygmomanometer and the sphygmograph are in apparent agreement, there are other instances in which they do not agree, and in which, indeed, the messages delivered by the two instruments relative to the level or height of the blood-pressure are directly and mutually contradictory.

In these circumstances, what is the unfortunate clinical instrumentalist to do? Possibly, as he contemplates the sphygmomanometer and the sphygmograph in the light of the records here exhibited, his feelings may be disposed to express themselves in the well-known lines:—

> "How happy could I be with either,
> Were t'other dear charmer away."

and he may, perhaps, even be impelled to the conclusion—

> "But as you both tease me together,
> To neither a word will I say."

If, however, he must, or thinks he must, make a choice—selecting one method and ignoring, as is the current fashion, the teaching of the other—shall he choose the sphygmomanometer with its attractive air of precision and accuracy, or shall he prefer the less precise but more picturesque displays of the sphygmograph; or, again, rather than either, shall he determine to fix his faith on his own trusty three fingers on the radial artery, and brush aside both sphygmomanometer and sphygmograph as equally useless and equally untrustworthy?

Now the suggestion I beg to offer in this dilemma is that our clinician shall not take any one of the three courses thus proposed to him, but that, on the contrary, while striving always for the "educated finger," he shall use *both* the sphygmomanometer *and* the sphygmograph; but I suggest, that in doing this, he shall apply to the records so obtained canons of interpretation somewhat different from those which are generally current. For, each of these instruments certainly gives us some information on the subject of blood-pressure. The sphygmomanometer, if it does nothing else, at least tells us what amount of pressure is necessary to overcome the pulsations of the brachial artery; and no one questions that the ascent of the sphygmographic wave coincides with, and is due to, a rise of intra-arterial blood-pressure, and similarly, that a descent of the wave represents, and is dependent on, a fall of such pressure. But as, *when tested by the claims usually made on their behalf,* these two instruments, though sometimes in apparent agreement, are often found in a state of open, conspicuous and flagrant contradiction, surely the reasonable and common-sense conclusion is that it is the "claims" and not the instruments which are at fault. And the solution of the mystery is to be found, I suggest, in a recognition of the truth that blood-pressure, as any other pressure, has other qualities than mere level or height, and that one of these is continuance or duration.

The measurement of the height to which blood-pressure rises is one question, the measure of the endurance of the pressure *in time,* is another and an entirely different question. Relatively high or relatively low, and independent of the fact of high or low, blood-pressure may manifestly be either prolonged and abiding, or brief and evanescent; whatever be the maximum attained, the decline therefrom may be either slow and gradual, or sudden and abrupt.

Now upon this second question—namely, the duration of the maximum pressure and the manner of pressure decline, the sphygmomanometer does not, and cannot, give us any information. It may announce the measure of the force of the ventricular systole as this reaches the brachial artery, but it certainly cannot announce the time during which the blood-pressure so caused continues, for, given a certain maximum of pressure within the brachial artery, the sphygmomanometric pressure necessary to neutralize or overcome this, will be one and the same, whether such maximum persists for a relatively prolonged period or whether, on the contrary, its duration is but for a moment.

On the other hand, the sphygmograph, whatever be its other values, certainly does not contribute any reliable information as to the maximum level to which the blood-pressure rises, for, as we have seen, with one and the same maximum record in different patients the pulse pictures may be of directly opposing types (fig. 26), and with different maximum records the pulse pictures may be of one and the same type (fig. 29). But as, without doubt, the descending line of the sphygmographic wave corresponds more or less closely to the movement of pressure decline within the artery, we must surely find on contemplation of this line some information as to the manner of such decline. In other words, if the maximum pressure, whatever it be, is relatively prolonged, and the decline therefrom is slow and gradual, the sphygmogram will announce these facts in a sustained wave, followed by a sloping or oblique downstroke (fig. 23, *A*); while, on the contrary, when the maximum pressure, whatever be its absolute reading, is promptly followed by a sudden fall, the sphygmographic wave will present a sharp apex and a sudden and abrupt descent (fig. 23, *B*, *C*).

If we admit these conclusions, and if, further, we recognize that as the sphygmographic picture conveys no information as to the height or level of the blood-pressure, the terms "high" blood-pressure and "low" blood-pressure applied to sphygmograms are misleading and inexact, and if, once again, we agree to replace these terms by the phrases "well-sustained blood-pressure" and "ill-sustained blood-pressure" respectively, all the apparent contradictions and mutual confusions of the sphygmomanometer and the sphygmograph cease and disappear.

The two instruments not only are not, but cannot possibly be, mutually contradictory, for they speak, not of the same, but of two different and independent things—the one of the level to which for an undefined period of time the blood-pressure rises, the other of the relative duration of the pressure and of the manner of its decline. Each instrument has its own value, each has its own function, and the information obtained from the one supplements that conveyed by the other; but the activities of these instruments are displayed not on the same but on different planes, and therefore cannot possibly come into mutual conflict and collision.

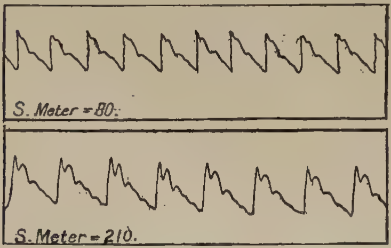

Fig. 29.—Sphygmograms of similar or identical pattern with very different (80 in the one case, 210 in the other) sphygmomanometer records.

It must be added that even when the sphygmomanometer and the sphygmograph have both been employed, there still exists a deficiency in our instrumental appreciation of the qualities of the blood-pressure, as neither of these instruments records the minimum level to which the intra-arterial pressure falls. To measure this so-called diastolic pressure many methods have been suggested, but none of them can be accepted with complete confidence, and the absence of this measurement lends emphasis to what is implied in the argument of this address—namely, that the customary announcement of the maximum sphygmomanometric figure on the one hand, or the presentation of a sphygmogram on the other, is anything but a complete, final, and exhaustive presentation of the clinical aspects of blood-pressure.

Whether I have succeeded in convincing you that the positions as I have presented them are well-founded and secure, I, of course, may not dare to say. But, thanks to your invitation, I have at least had the opportunity of stating these positions, and I now, with great respect, leave the verdict upon them to the decision of your better judgment.

ON THE OCCURRENCE OF PYREXIA IN CANCER AND OTHER DISEASES OF THE LIVER AND IN CASES OF GALL-STONE.

THE general opinion on the course of the temperature in cancer of the liver has probably been largely determined, directly or indirectly, by the statements in Murchison's well-known book on "Diseases of the Liver" and by the parallel passage in Wunderlich's elaborate work on "Medical Thermometry." According to Murchison, "in cancer, unless there be some inflammatory complication, the bodily temperature is at or about the normal standard, and accordingly in the case of an obscure internal disease a continuous elevation of the temperature would in itself be opposed to the diagnosis of cancer"; and he mentions, as quite exceptional events, two cases which departed from this rule. In one, the average morning record for the six days before death was 99.3° (maximum 101.1°) and the average evening record 101.6° (maximum 103°), this condition of the temperature, together with other circumstances, inclining the diagnosis in the direction of pyæmic inflammation. The second case is simply noted as one of "primary uncomplicated cancer of the liver with a temperature of 102°." Wunderlich's opinion is that "it is a peculiarity of cancer cases that elevated temperatures are comparatively rare and that the temperature generally maintains itself on a normal or subnormal plane, which, however, by no means precludes the occurrence of high temperatures through intercurrent complications or at the close of the disease; but fever temperatures of long duration are at least rare in cancer patients."

Some other authorities may also be quoted on this point.

Harley says: "All signs and symptoms of pyrexia are in cases of non-complicated cancer of the liver totally absent."

Hilton Fagge: "There is sometimes slight pyrexia (100° or 101° and in one case [Pye-Smith] 104.6°) for which no other cause can be found after death."

Budd: "When the tumours grow rapidly some degree of fever is set up."

Frerichs: "Progress, however, is never completely uniform . . . periods during which the tumours grow rapidly, the pains increase and fever shows itself, are followed by others in which the morbid growth is quiescent or retrogrades."

Jenner: "A large amount of cancer may be rapidly formed without any elevation of temperature."

Osler: "In all rapidly-growing large neoplasms of the liver there is more or less fever, usually continuous and not reaching a high grade."

With two exceptions these authorities may be cited to show, that, whatever be the explanation, pyrexia more or less extreme is an event which may undoubtedly appear in the course of a case of hepatic cancer. In reference to the phrase "non-complicated cancer of the liver," which is used by several authorities, it may be remarked that while the suggested limitation may be of importance to the issue in an academic discussion, it is of little value from the practical and clinical point of view unless the existence of "complications" can be definitely and confidently determined. And it is notorious that, as a matter of fact, this is not the case. Not only, as stated above and as will likewise appear in the sequel, are there cases of cancer with accompanying pyrexia in which after death no "complication" can be discovered, but it is also common to find after death in the neighbourhood of a cancerous liver inflammatory changes of whose existence during life there was an entire absence of any suggestive evidence. Further, there is the knowledge that the occurrence of such inflammatory changes in conditions other than cancer —as, for example, from the irritation due to a long-impacted gall-stone—produces in the examination of the patient facts which are the very crux of the physical diagnosis. The problem at the bedside is not, Does cancer pure and simple cause pyrexia? but rather, What is the interpretation of the physical facts in this case, and what bearing has the state of the temperature on the diagnosis? If the physician is able to pronounce the case to be one of cancer, it is of little practical moment whether he believes any existent pyrexia to be due to "complications," or to be a direct result of the malignant process: there is not here any diagnostic difficulty. Such difficulty arises when—and it is not an exceptional experience—the physical facts are of doubtful interpretation, and when, therefore, the weight and bearing of the other features of the case, including the state of the temperature, have to be carefully reviewed and estimated.

The clinical question, in short, is not, Is the case one of cancer with or without complications? but, Is the case one of cancer at all? And it is the ambiguity of the physical facts, which, in not a few cases of hepatic disease, raises, in such cases, to a level of high importance the study of the value and significance of the symptomatic and general evidence. Of course, if it is true, as it undoubtedly is, that a considerable degree of pyrexia may attend cancer of the liver, the clinical distinction of this disease from impacted gall-stone, abscess, and other hepatic diseases in which febrile disturbances more or less frequently occur, is not facilitated. But the recognition of this clinical truth may at least save the physician from an inclination to place undue stress upon the existence of pyrexia as a reason for the summary exclusion of cancer from the diagnosis.

PYREXIA IN CANCER OF THE LIVER.

The facts as to temperature in some cases of cancer of the liver are displayed in the following charts. In Case 3 there was no necropsy, but in the other instances the diagnosis was confirmed by post mortem examination.

CASE 1.—The patient was a man, aged 48. His illness commenced two months before the date of the chart with "attacks of cold and shivering," after which he became jaundiced; there were no rigors while the

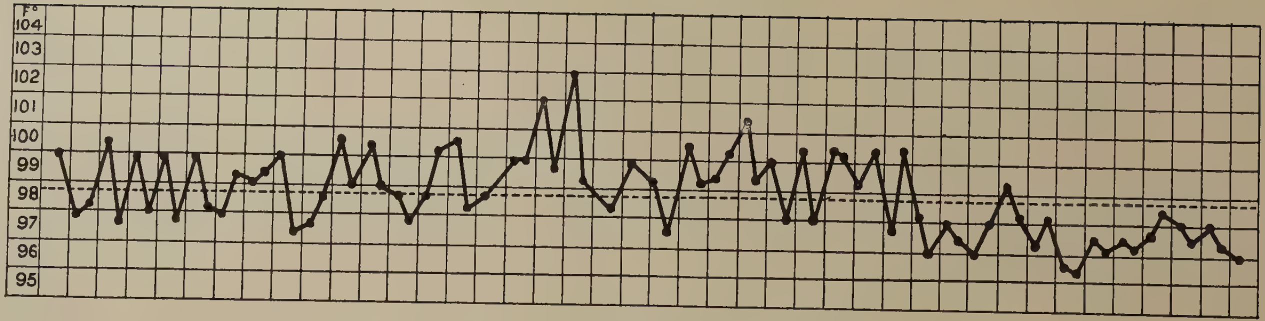

CHART I.—Moderate and prolonged pyrexia in a case of cancer of the liver.

man was under observation. The physical facts were highly suggestive. Towards the end, fluid accumulated in the cavity of the peritoneum and there was œdema of the lower limbs. The necropsy showed numerous carcinomatous tumours in the liver. On the surface of one of these was a red hæmorrhagic area with a brown coagulum in the neighbourhood, and the peritoneal cavity contained a large quantity of blood—some manifestly recent, some of older date. The occurrence of hæmorrhage within the cavity of the peritoneum is of interest. Murchison speaks of this as "a rare mode of fatal termination of cancer of the liver" and records one case. Frerichs, also, records a case in which the abdomen contained about 12 lbs. of blood, and figures the liver with two cancerous nodules which had perforated the peritoneum in the form of soft cauliflower excrescences covered with recent blood-coagula. In Murchison's patient death occurred somewhat suddenly; in Frerich's case the hæmorrhage probably commenced about three days before death; whilst in the instance here recorded the condition of the clots showed that the hæmorrhage had been a much more chronic process. It may well have extended over the last fortnight, when, it will be observed, the temperatures were for the most part subnormal.

Case 2.—The diagnosis in this case was very obscure, and subphrenic abscess, abscess of the liver, and cancer of the liver, were all discussed. In consequence of the physical facts existing in the right chest, paracentesis was twice performed, on the first occasion with negative results, and on the second with the withdrawal of a large quantity of a creamy fluid. This fluid was proved after death to have come from a large cancerous cyst (fig. 41), which, growing from the upper surface of the right lobe of the liver, had pushed up the diaphragm and compressed the lung. Though the patient had not complained of gastric symptoms, there was a malignant ulcer in the wall of the stomach. The temperature was a confusing element in the diagnosis.

Case 3.—In this case there was little or no doubt about the diagnosis, and the patient died some six weeks after leaving the hospital. The degree of fever is but slight, yet the chart is not without interest as an example of pyrexia in cancer and on account of the tendency, more marked in the earlier records, to a decided rise of the temperature in the evening.

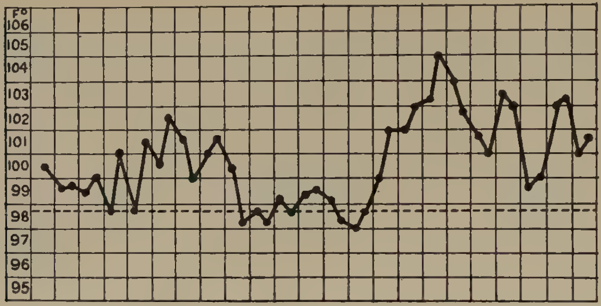

CHART 2.—Considerable febrile disturbance in a case of cancer of the liver.

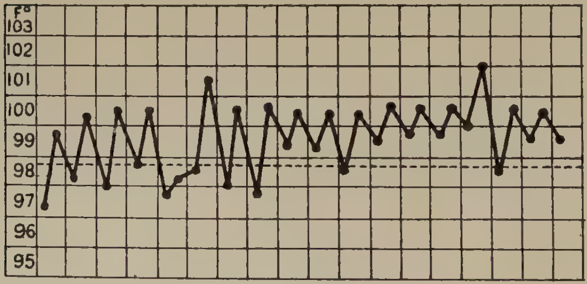

CHART 3.—Moderate but sustained fever in a case of cancer of the liver.

CASE 4.—The patient was a man of 56 years. He had been gradually losing strength and flesh for six months, and had occasionally vomited. Jaundice had been present for four months, and was very pronounced. At no time had pain been a feature of the case. The liver was moderately enlarged, and there was evidence of distension of the gall-bladder. As the chart shows, the patient suffered from a series of brief intermittent

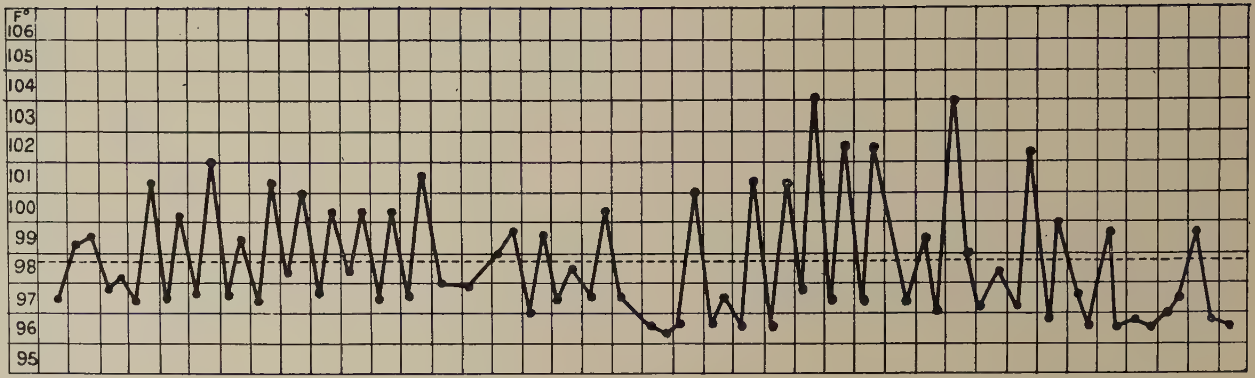

CHART 4.—Considerable and prolonged pyrexia of intermittent type in a case of malignant ulceration of the duodenum and cancer of the liver.

pyrexial attacks, each of the most severe being preceded by a rigor. The jaundice was extreme, the skin being of a deep olive tint. At the post mortem examination a malignant ulcer, about 1½ in. in diameter, was found in the duodenum. About the centre of the ulcer was the opening of the common bile-duct, which was greatly dilated. The cystic duct was blocked by a mass of tumour tissue, and the gall-bladder was distended with a quantity of milky-looking fluid, which under the microscope showed numerous fat globules and leucocytes. Scattered throughout the liver were numerous small, softening, cancerous nodules, and a mass of enlarged and indurated glands was adherent to the duodenum.

In this case it is not the mere occurrence of pyrexia which is of chief interest, but rather the course and character of the pyrexia, and the fact that on many occasions each of the sharp, short-lived outbreaks of fever was preceded by a severe rigor. And the interest has a marked practical aspect when it is recollected that pyrexial disturbances of exactly this character, and with identical attendants, are often to be recognized in cases of impacted gall-stone (see Chart 5); that cancer and gall-stone alike frequently cause jaundice and pain; that, as already suggested, the physical evidences in cancer are frequently non-characteristic; and that an impacted gall-stone, by producing hepatic congestion with peritoneal thickening and matting together of neighbouring tissues, may be attended by physical signs which are ambiguous or misleading (see Case 5). In this connection it is of importance to note the complete absence of pain from the above case, and the value of this in the exclusion of gall-stone from the diagnosis. That the gall-bladder was distended also pointed to malignant disease, the association of jaundice with such distension being in Mayo Robson's very special experience significant of the more serious malady. The same author also lays down the rule that the "absence of ague-like attacks" favours a diagnosis of malignant disease. It is well to note that this statement does not imply that the presence of such attacks is conclusively in favour of gall-stone. The above case shows that such a proposition could not be sustained, and among Mayo Robson's records there are several instances in which jaundice due to malignant disease was associated with rigors, intermittent fever, and ague-like seizures.

In Murchison's list there is a case (CXLII) almost identical with the one just recorded, and here also in the later stages there were rigors, pyrexia, and sweatings. Rather curiously, too, in view of what is stated above, there were physical signs as of a dilated gall-bladder, but found after death to be caused by a malignant tumour which "occupied the situation of the gall-bladder, but was unconnected with it, having originated in the coats of the duodenum." Though not strictly to the present purpose, it is difficult to avoid remarking that in this case jaundice, which had existed for nine months, almost entirely disappeared two months before death. This very unusual occurrence in jaundice from malignant disease appears to have been due to sloughing of the tumour which had previously blocked the orifice of the common duct. On this point a very important remark is made by Mayo Robson—namely, that jaundice in cancer may be due to catarrh of the biliary passages, and so may be relieved by treatment, without, of course, any benefit to the original disease. Considering that in a balanced diagnosis between gall-stone and cancer, disappearance of the jaundice would weigh strongly in favour of the former, the qualification of this conclusion implied in Mayo Robson's observation is of some moment.

But it may be said that cases such as Murchison's, and the one described above, cannot fairly be quoted in support of the statement that a considerable degree of pyrexia may accompany cancer of the liver, seeing that in each there was in the intestine an ulcerated surface through which septic poisoning may easily have occurred. It may readily be allowed that the febrile disturbances possibly originated in this way, and it may even be conceded that the cancerous changes in the liver had no actual part in the production of the pyrexia.

Yet it remains true that in consequence of the latency of the duodenal ulcer, of the existence of jaundice, and of the discovery of certain physical facts in the examination of the hepatic region, the cases as clinical problems were cases of hepatic disease with jaundice. And it thus was necessary in each instance to compare the facts, including the existence of pyrexia, with the various diseased conditions of the liver possibly competent to produce them. A failure to recognize that the pyrexial phenomena, whatever be the theory of their production, were quite consistent, in such cases, with the existence of cancer of the liver, would have led to an error in diagnosis. When it is known that there is a primary malignant tumour in the uterus, breast, alimentary canal, or elsewhere, the development of jaundice and of enlargement of the liver, whether accompanied by fever or not, has an interpretation only too manifest. But when no such knowledge exists, and, as at least three of the present cases show, this is a frequent position, such events as jaundice and enlargement of the liver are open to various explanations. Hence the necessity for recognizing that neither a distinct degree of pyrexia, nor the occurrence of rigors and intermittent febrile attacks, is by any means conclusive against one of these possible explanations, namely, malignant disease.

The two statements, that a malignant process which commences in the gastro-intestinal tract frequently comes into practical view only when secondary tumours have formed in the liver, and that hepatic cancer may be associated with considerable febrile disturbance, are well illustrated in Cases 2 and 4 above reported, and also in two cases recorded by Finlayson. In one of these, together with feverish symptoms, there was such a degree of bulging or swelling of the liver that an operation was undertaken in the expectation of finding pus. The post mortem examination revealed malignant growths in the liver and a large sloughing ulcer of the stomach. In the second case the patient had jaundice, and for ten weeks there was remittent fever; after death a cancerous ulcer was found in the duodenum and numerous malignant tumours in the liver. As a further demonstration of pyrexia in malignant disease, the same author also reports a case of epithelioma of the colon, the early symptoms of which closely resembled the onset of enteric fever; later, there were definite rigors, for the occurrence of which the post mortem examination gave no explanation other than the malignant tumour. From these and other cases Finlayson concludes that rapidly-growing or rapidly-diffused cancers are often associated with a distinct but moderate pyrexia.

Hale White has suggested that pyrexia is more common in primary than in secondary cancer of the liver, but the extensive series of observations necessary to raise this suggestion to the level of established doctrine has yet to be made, and considering the frequency with which secondary cancer is associated with the existence of an ulcerated surface in the gastro-intestinal tract, one would expect pyrexial manifestations here rather than in primary cancer, where no such opportunity for septic absorption exists. In Murchison's case (CXLI), and in several of Finlayson's cases, as well as in Cases 2 and 4 of the present paper, the facts, so far as they go, are in harmony with this expectation.

The occurrence of intermittent febrile attacks in cases of hepatic disease was noted by Charcot, and in his "Maladies du Foie" he devotes a special chapter to what he calls "fièvre intermittente hépatique." The circumstances which favour the production of this condition he considers to be the presence in the dilated biliary passages of pus or muco-pus mixed with the bile in a state of stagnation. Nevertheless, the intermittent fever may exist, he says, when there is no suppuration, and he then attributes it to the presence in the inflamed and dilated biliary passages of a morbid poison resulting from an alteration in the bile. It is particularly to be observed that Charcot does not confine the occurrence of the intermittent fever to cases of gall-stone. He says distinctly that it may be found in any case where there is a prolonged or permanent obstruction in the bile-duct, such, for example, as fibrous stricture or cancer of the head of the pancreas.

Similarly, Murchison writes: "The paroxysms of fever are comparatively rare in obstruction of the bile-duct other than from gall-stones. Still, I have observed them in two or three instances of catarrhal jaundice, and also in cancerous stricture."

Pyrexia in Cases of Gall-stone.

Case 5.—The patient was a man of 32 years. When first seen he had been jaundiced for fifteen months, during which time he had had a number of attacks of severe paroxysmal pain. There was considerable enlargement of the liver, which was tender to palpation but without any appreciable nodulation; little or no evidence of distension of the gall-bladder. The spleen was definitely enlarged. Under observation the temperature pursued the above course (Chart 5), some of the febrile attacks being preceded by rigors and accompanied by epigastric pain, others occurring without either rigors or pain. There were some attacks of pain without accompanying rise of temperature. After each febrile outbreak there was sensible deepening of the jaundice. The case was regarded as one of impacted gall-stone, and an operation was performed, but no calculus was discovered. Nine months later the patient was seen with evidences of pleural effusion and

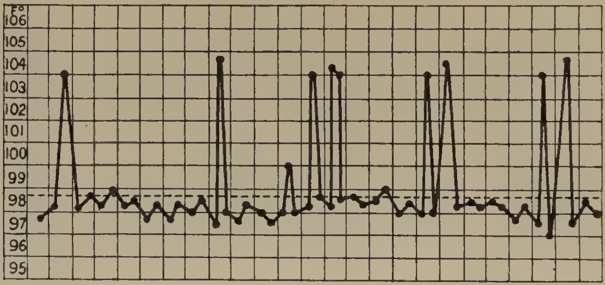

Chart 5.—Irregular, short-lived febrile attacks in a case of impacted gall-stone.

pulmonary condensation at the base on the right side. The jaundice remained unchanged, and there was considerable enlargement of the liver and spleen, with a widely-diffused sense of irregular thickening and resistance in the upper abdomen. The patient died in the course of a few days, and a large gall-stone was found impacted in the orifice of the bile-duct, part of the calculus protruding into the duodenum. There was considerable dilatation of the biliary passages, and the liver was highly congested but otherwise normal. In the neighbourhood of the liver, spleen, stomach, and transverse colon, there was extensive peritoneal thickening, and numerous fibrous adhesions existed between these organs.

The age of the patient, the history of the illness, the physical facts, and the irregularly intermittent temperature, combined to urge a diagnosis of gall-stone. Unfortunately, the calculus was not detected at the operation, and this failure, together with the discovery by the surgeon of some " nodules " in the neighbourhood of the cystic duct, rendered a suspicion of cancer inevitable. When the patient was seen a short time before his death this suspicion appeared to receive strong confirmation from the physical conditions discovered on palpation of the upper abdomen. Yet these, equally with the jaundice, were the consequence of gall-stone, though, as just pointed out, they seemed to support a diagnosis of cancer. That—at least, under the circumstances just described—the physical examination in a case of impacted gall-stone is capable of affording such support is not the least instructive feature of the case.

Many observers have written upon the occurrence of paroxysmal febrile attacks in cases of gall-stone. A quite characteristic case is quoted by Sir Jonathan Hutchinson from Richter's "Medical and Surgical Observations." The patient, a man of 40, died in the Hospital at Goettingen in 1792, and a gall-stone weighing 3 oz. and 5 dr. was found in the common duct. For four years the patient had had jaundice with attacks of colic, rigors, and fever. Before death bile returned to the stools, and at the post mortem examination fluid bile was found all round the calculus, so that the case partially illustrates what has been described as the "ball-valve" action of a gall-stone, by which, in spite of the presence of a stone, the occasional passage of bile into the duodenum is permitted. The explanation which Charcot offered of the paroxysmal fever in these cases has already been quoted (p. 193). Murchison allowed that in some cases the fever might result from ulceration of the duct leading to pyæmic inflammation of the liver, but he contended that as in some instances "the fever ceases and the patient gets perfectly well," this was not the invariable explanation. "It may," he writes, "be due to a movement of the stone in the narrow duct, just as . . . a paroxysm of fever may follow the introduction of a catheter into the urethra." Budd, Thudichum, Lionel Beale, and Ord, all inclined to the view that the rigors and fever are results of nervous disturbance produced by the calculus irritating the mucous membrane and stretching the walls of the ducts; and it is certain that considerable reflex and nervous disturbance may be produced by the irritation of a calculus, as is shown by the occasional occurrence of muscular cramps and convulsions in association with hepatic colic. Mayo Robson, on the basis of his own experience, believes that the fever may be an expression both of a septic element and of nervous irritation. Sir Wm. Osler records two cases of stenosis of the common duct without fever or rigors as sufficient to show that mere obstruction will not cause these events. He allows that the condition of the biliary passages in cases of impacted gall-stone is usually one of catarrhal, not of suppurative, cholangitis, but points out that the discovery of micro-organisms in both these conditions renders it probable that the fever now in question is due to absorption of a ferment.

From the occurrence of intermittent febrile attacks and jaundice, with physical signs of enlarged liver, both in malignant disease and in impacted gall-stone, the differential diagnosis of these two conditions becomes an important practical question, all the more so that modern surgery has rendered the relief of one of them a comparatively secure undertaking. Attention has already been drawn to the fact that, as in Case 4, evidence of a distended gall-bladder favours the diagnosis of malignant disease. Murchison laid considerable stress upon the statement that when the jaundice becomes more intense after each febrile paroxysm it depends upon gall-stone. Sir Wm. Osler confirms Murchison's observation, and believes that intermittent fever, jaundice deepening after the attack, more or less pain, and gastric disturbance, combine to indicate quite clearly obstruction of the common duct by a calculus. Increase in the intensity of the jaundice after each febrile explosion was quite appreciable in Case 5 (gall-stone), and there was considerable pain; in Case 4 (cancer) the jaundice was so deep that it was impossible to imagine further exaggeration, and there was practically no pain. But it is to be noted that in this case the paroxysms occurred almost daily, and it is certain that they may be equally frequent in gall-stone, a circumstance which would allow but little opportunity for appreciating variations in the depth of the jaundice, supposing this to be aggravated by each repetition of the event which determines the fever. Further, it is, of course, not unusual to have pain with malignant disease, while in gall-stone the pain is not always characteristic or even suggestive, as is well seen in a case described by Gairdner. It may even be entirely absent, as in a man æt. 69, who, after jaundice of six months' duration and without pain, tumour, or vomiting, was entirely relieved by cholecystotomy and the extraction of forty-three stones (Sir Henry Morris). In a case of gall-stone reported by Chadwick there was only a solitary attack of biliary colic, this being followed by jaundice, which persisted until death, six years later.

From these experiences it is evident that a combination of jaundice, intermittent fever, and hepatic enlargement may demand very careful consideration before deciding to cast the verdict for gall-stone on the one hand or malignant disease on the other. Of course, there will be cases in which the patient's age, history, or general condition, or some conspicuous physical fact, will make the diagnosis distinct, but in other cases the problem must be an anxious and difficult one. The knowledge, too, that cancer and gall-stone may co-exist, with a correspondingly complex symptomatology, necessarily adds to the difficulties of the situation.

The development of rigors and intermittent fever as a result of gall-stone is not limited to cases accompanied by jaundice. Just as hepatic colic may occur without jaundice, as the result of the successful passage of small stones, or in consequence of the impaction of a calculus in the cystic duct, so, in similar circumstances, there may be intermittent fever; and in some of these cases pain is slight or even absent. Murchison's statement is that "now and then a gall-stone in one of the biliary ducts excites paroxysms of intermittent fever with little or no pain. These paroxysms may be more or less periodic, and may extend over several months without necessarily indicating pyæmic hepatitis, and the patient may ultimately recover." He draws attention to a case of Howship's in which, while spasmodic pains with ague-like attacks had been present for six months, the patient had been jaundiced—as a result doubtless of the onward movement of the calculus into the common duct—only during the last month of life. The preparation, showing a large calculus fitted tightly into the end of the duct, is in the Museum of the Royal College of Surgeons. In Murchison's own series, too, there is an analogous case (CXXXI). Waring, also, describes the occurrence of rigors from gall-stone without the development of jaundice. From such cases, and especially from the recovery of some of the patients after the febrile paroxysms had been present over a prolonged term, some support may be derived for the "nervous" as opposed to the "septic" explanation of the fever. Thudichum's view was that intermittent febrile attacks were the peculiar feature of calculi in the hepatic duct or ducts. "When so situated," he says, "calculi produce symptoms of a peculiar kind, differing from the symptoms of gall-stones in the gall-bladder and common duct by the absence of pain, and having the greatest resemblance to chronic intermittent fever." And again, "as these calculi are rarely moved from the place in which they are formed, and as their formation is so gradual as to effect a corresponding expansion of the canal, they rarely produce symptoms of the passage of calculi and do not easily give rise to jaundice." It will surely be allowed that the formation of calculi on the proximal side of the gall-bladder is anything but a frequent occurrence, and certainly the occurrence of rigors is not restricted to calculi so situated, as Case 5 (p. 194) and other similar instances definitely show.

One other remark falls to be made about the temperature in gall-stone—namely, that the fever, instead of being intermittent, may be of a more sustained character, and may continue for some time after the pain has ceased. This condition appears to be associated with wounding or ulceration of the mucous membrane produced by the passage of the calculus (Murchison, Krauss). Trousseau considered that when hepatic colic has been of long duration or has recurred at short intervals, the feverish condition of the patient is due to inflammation of the liver.

The Temperature in Hepatic Abscess.

In the various forms of abscess of the liver, rigors and elevation of the temperature, remittent or intermittent in character, are to be expected. But this expectation is not always realized, as the following case shows.

Case 6.—The patient, a Lascar, had suffered from abdominal pain and diarrhœa for three weeks. The pain was referred to the left iliac and hepatic regions, and there was some tenderness in the latter situation, but without evidence of distinct enlargement of the liver. The stools were frequent, fluid in character, and contained small shreddy masses like blood-clot. Temperature as in Chart 6; no rigors. At the post mortem examination there was found extensive destruction of the mucous membrane of the colon, with firm adhesions between the bowel and neighbouring parts. The liver contained four abscesses—three each about the size of a walnut, and a smaller one.

Murchison records a case (LXXIV) of gall-stones with ulceration and perforation of the cystic duct and numerous abscesses in the liver, but in which there were no rigors, and the temperature never exceeded 98°, the patient being under observation for five days before death. He adds: "I was scarcely prepared to meet with pyæmic inflammation of the liver with no elevation of temperature during at least five successive days." Murchison also reports the occurrence (LXXXVI) of an enormous tropical abscess of the liver without any elevation of temperature, and, while recognizing the case as exceptional, says: "I am inclined to think it is far from unique."

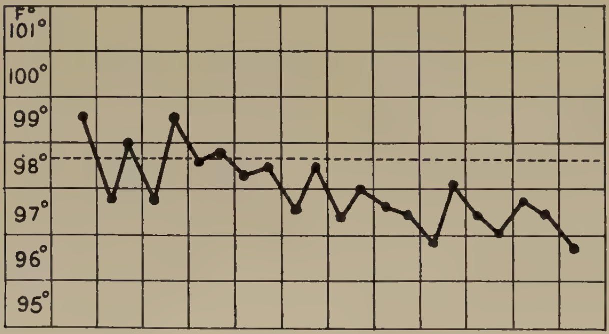

CHART 6.—Absence of Pyrexia in a case of Abscess of the Liver.

TEMPERATURE IN HYDATIDS OF THE LIVER.

Intermittent febrile attacks with jaundice and pain have been observed in hepatic diseases other than cancer, gall-stone, and abscess. Identical symptoms have followed the bursting of a hydatid cyst with the passage of secondary cysts into the bile-duct (Frerichs, Trousseau, Murchison, Mayo Robson). The same association of events has followed the entrance of lumbricoid worms into the biliary passages (Frerichs, Trousseau, Murchison).

TEMPERATURE IN CIRRHOSIS OF THE LIVER.

CASE 7.—The patient was a man, aged 34, who was believed to be the subject of cirrhosis of the liver. The history was of long-continued spirit drinking with recent failure of general health, two or three attacks of vomiting of blood, and frequent passage of blood *per rectum*. Patient's face was sallow but not jaundiced. The urine yielded a copious deposit of pink urates but was free from bile pigment. The spleen was considerably enlarged, and the area of hepatic percussion, if anything, below normal; no hepatic pain or tenderness. There was some dilatation of the superficial veins over the right half of the abdomen but no ascites. The bowels were loose, and the stools contained a quantity of blood, which was so intimately mixed as to give a uniform darkish-red colour to the motions: no hæmorrhoids could be detected. The high temperature, the diarrhœa, and the presence of blood in the stools gave rise for a time to a suspicion of the possibility of enteric fever, but there was no further evidence to sustain this. The patient was seen again a year after he had left the hospital. His general condition was much the same and the physical facts substantially unchanged. The temperature showed a decided pyrexia. He had had only one further attack of hæmatemesis. There was no suspicion of malarial infection.

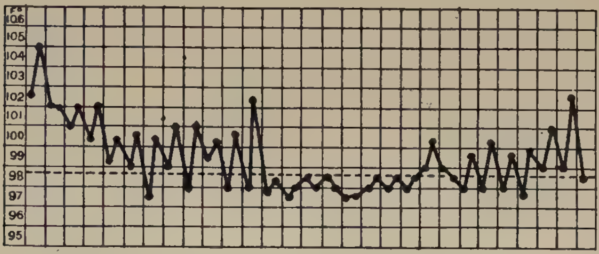

CHART 7.—Pyrexia in a case of Cirrhosis of the Liver.

Pyrexia is not usually recognized as one of the results of hepatic cirrhosis. Yet its occasional existence has been noticed by various observers. Murchison, in his account of the symptoms, only mentions the existence of fever as attending an occasionally acute onset of the disease, or as accompanying vomiting and diarrhœa from catarrh of the stomach and bowels. In one (CXVI) of his cases, however, there is a note of a temperature of 101.2°, and in another (CXXII)—a boy of 9 years—the temperature was at times as high as 100.6°, and among the evening records was one of 102.8° and one of 103.4°.

Frerichs, also, speaks of an occasional febrile onset of the disease, and mentions "slight febrile excitement" as one of the possibilities of the later stages of its progress. In reviewing the etiology of the thirty-six cases which he had observed, he says: "In five patients the cirrhosis was preceded by persistent intermittent fever —a coincidence which has been repeatedly mentioned by other observers."

Carrington reports, from the clinical records of Guy's Hospital, the occurrence of irregularly febrile temperatures in eighteen out of forty-four cases of cirrhosis of the liver. He also describes a case of hypertrophic cirrhosis in which the symptoms for four weeks before death "completely simulated" those of multiple hepatic abscess.

According to Webber, in a series of fifty cases of cirrhosis of the liver, pyrexia was present in nine; in three the temperature rose to 103°, and in one of these the pyrexia was of an intermittent type.

Hawkins recognizes the separation of alcoholic cirrhosis into two classes, namely, multi-lobular and uni-lobular. The latter and less common variety is marked by considerable and even extreme enlargement of the liver, by a great tendency to jaundice, and by little or no evidence of portal obstruction. "There is often an evening rise of temperature . . . . the fever may range from 102° to 104° . . . . and it may assume a hectic course suggestive of hepatic abscess or pylephlebitis." In the multilobular or atrophic form, on the other hand, there is, as a rule, no fever, though "if a patient be under observation for a long time, we may often observe an occasional rise of two or three degrees for a few successive evenings."

Sharkey has recorded a case of uni-lobular cirrhosis in which there was much and irregular fever, and which exactly corresponded to the above quoted description, except that jaundice was slight, and disappeared entirely before death. No explanation of the pyrexia was found at the post mortem examination. Charcot, also, describes the development of jaundice, often accompanied by fever, in hypertrophic cirrhosis. Writing on this subject Sir Wm. Osler says: "I have a distinct recollection of one or two instances, particularly in the hypertrophic form, of marked increase in the temperature, but I should say that in the majority of instances of cirrhosis pyrexia was not a special symptom."

The verdict of the authorities who recognize the existence of two varieties of cirrhosis is in favour of the occurrence of pyrexia in the hypertrophic rather than in the atrophic form. The case above recorded (Case 7) has the peculiarity that whilst it follows the multilobular or atrophic type—the liver not enlarged, distinct evidence of portal obstruction, no jaundice—it is associated with considerable pyrexia, irregular in its course no doubt, but having generally the hectic character. There is also this noteworthy feature, that the man, seen after an interval of a year, had not to any great extent declined in health, and that he again showed quite decided pyrexia. The lapse of time in itself seems more than sufficient to exclude any suppurative process; the absence of tenderness negatives the suggestion of perihepatitis, even if the element of time is neglected; and similarly, the absence of jaundice excludes, or at least renders highly unlikely, the possibility of those conditions, whatever they be, which are associated with obstruction of the biliary passages and thus produce "intermittent hepatic fever." It seems, therefore, that cirrhosis of the liver, even when of the atrophic or multilobular type, must be added to the numerous clinical events capable of producing considerable and indefinitely prolonged pyrexia by a method or agency of unknown nature.

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ON ENLARGEMENT OF THE SPLEEN IN CANCER OF THE LIVER.

THE physical facts in some cases of cancer of the liver are so conspicuous and unequivocal that the diagnosis is rendered both inevitable and confident. On the other hand, there are numerous cases of the disease in which the physical examination is quite inconclusive, either because no sensible departure from the normal can be appreciated, or—and this is the more frequent event—because the facts discovered are of uncertain significance. Now and then the physician no doubt does see a patient in whom the classical features of cancer of the liver are presented in unmistakable characters. But in the majority of instances the diagnosis is merely a more or less confident inference founded partly on general symptoms and other considerations, and partly on physical facts of admittedly uncertain import. Experience in this, as in many other departments of medicine, discovers the frequent absence of what is usually called "typical evidence," and this remark applies both to the facts of physical examination and to the general symptomatology of the disease. It has to be recognized that cancer of the liver may exist without nodulation, without pain, without tenderness, and even without enlargement of the organ, just as it may—and indeed very frequently does—pursue its entire course unaccompanied by either jaundice or ascites. And yet again, physical or other evidence may render the diagnosis of cancer morally certain, although the patient may, and that too for a comparatively prolonged period, experience but little sense of prejudice to his general health. It is therefore practically impossible to rely upon the absence of any individual symptom or physical sign as valid ground for the exclusion of a diagnosis of cancer of the liver. This diagnosis must rest upon a review of all the facts of the case, and may quite well be justified even though one or more of the characteristic features of the disease be entirely absent. Further, it is necessary to remember, what experience soon makes commonplace, namely, that cancer of the liver as a clinical condition, is, even in cases not doubtful, attended by physical facts and general symptoms common to it and other diseased states of the liver. Hepatic enlargement is, of course, a consequence of many morbid processes; nodular projections on the surface of the liver may quite well be syphilitic, and, if only a single one is present, abscess or hydatid cyst is, equally with cancer or gumma, a possible explanation; and tenderness on palpation is present in congestion, in cancer, and in many other conditions liable to be marked by the supervention of perihepatitis—even amyloid disease cannot be absolutely excepted from this statement. Similarly, jaundice, ascites, local pain, general prostration and loss of flesh, either singly or in combination, may be associated with physical signs of enlargement of the liver from several conditions other than cancer.

It is not, then, on this fact or that fact that the diagnosis can be rested, but upon the grouping of the facts, and the cases are not infrequent in which, at least for some considerable time, there is difficulty in deciding on which side the probabilities lie.

In these circumstances it is not surprising to find a welcome given to any piece of evidence for which diagnostic value is claimed, even though the only precise significance attached to such evidence is its power to exclude the diagnosis. Enlargement of the spleen, is often claimed to possess this clinical value. It is not infrequent to find statements, almost absolute in their terms, to the effect that this event excludes cancer of the liver from the diagnosis. No doubt such statements are, for the most part, less the outcome of original observation than the presentation in an abbreviated form, and separated from their context, of generalizations framed by competent authorities on the basis of a large experience. Nevertheless, the conclusion above quoted receives so frequently the support afforded by individual cases, that experiences which show its command to be not an absolute one become of considerable clinical moment.

The statement that the spleen is not enlarged in cancer of the liver, is obviously sharpened for diagnostic purposes by the frequency with which such enlargement occurs in connection with other hepatic diseases. Thus both organs may be affected with amyloid disease; both may be enlarged in chronic malarial conditions; a splenic tumour is frequent in cirrhosis of the liver; in tertiary syphilis, both organs may be enlarged from amyloid disease, or gummatous formations in the liver, with thickening of the fibrous capsule of the organ, may be attended with passive congestion of the spleen; hydatids of the liver are often complicated with hydatids of the spleen (Budd,[1] Frerichs,[2] Murchison[3]); in multilobular hydatid tumour of the liver, splenic enlargement is quoted as one of the diagnostic signs (Nothnagel,[4] Murchison, Waring[5]); in pyæmic abscesses affecting the liver, " the spleen," says Murchison, " is frequently enlarged, owing, not to obstructed circulation, but to the tendency of that organ to enlarge in consequence of the morbid condition of the blood "; according to the same authority, splenic enlargement does occasionally attend tropical abscess of the liver; and lastly, in cases of long impacted gall-stone, it is not uncommon to find some, and even considerable enlargement of the spleen (Mayo Robson,[6] Chadwick,[7] Waring). Many of these conditions are, doubtless, at least in the majority of instances, attended by evidences which make their differential diagnosis from cancer a comparatively easy matter. Yet it will scarcely be denied that individual cases arise in almost every one of these conditions in which such diagnosis is attended with great difficulty. At all events it must be granted—(1) that in a certain proportion of cases in which there is no doubt at all that the liver is diseased, the physical facts and other features of the case are difficult of interpretation; and (2) that at least a large proportion of the above-mentioned diseases of the liver have, at one time or another, been mistaken for cancer, or *vice versâ,* by observers of no mean experience and competence. And probably every physician who has had much practical acquaintance with cases of hepatic disease has reason to recall such precedents for his own consolation.

The exact statement of Frerichs[8] on the condition of the spleen in hepatic cancer, is that "it is very rarely enlarged; of the ninety-one cases subjected to analysis there was enlargement of the spleen in only twelve; in all the other cases the organ was either of normal size or small." This statement has often been quoted, sometimes in its entirety, sometimes in its first clause only. Again, there are writers who state that the spleen is *never* enlarged in cancer of the liver, and others make no reference to the subject. Waring (*Op. cit.*) says enlargement of the spleen is absent in all "uncomplicated cases" of cancer of the liver. In the instances of hepatic cancer related by Murchison, only one (Case CI) presented a splenic tumour, and in this the lower end of the spleen was felt projecting for 1½ in. below the ribs. Post mortem examination showed the spleen to weigh 17½ ozs., and to contain several masses of cancer. In this case the right kidney, as well as the liver and spleen, was affected, but clinically the case was one of cancer of the liver; for, in addition to hepatic and splenic enlargement, the symptoms were jaundice, ascites, and abdominal discomfort; there was an entire absence of renal disturbance, and the urine was free from albumin, although the right kidney was found after death to be "simply a mass of disintegrating cancer." Jenner,[9] after stating that "cancer of the spleen is exceedingly rare," quotes a case in which, following scirrhus of the breast, the liver became large and hard, and there was present in the left hypochondrium a large tumour, "plainly the spleen."

It may then be accepted that enlargement of the spleen in cases of cancer of the liver is quite an unusual experience. There are, however, occasional exceptions to this rule; and further, there are at least two other possible events either of which may readily be mistaken for enlargement of the spleen, and may thus, when associated with cancer of the liver, assist in producing an erroneous diagnosis. The first of these is the simulation of a splenic tumour by an enlargement of the left lobe of the liver; and the second, the development of a cancerous mass contiguous to the spleen, though not actually involving it.

A case illustrating the former of these two possibilities was that of a man who was the subject of an intrathoracic tumour, probably non-aneurysmal. In the left hypochondrium was a firm mass which extended downwards and forwards from below the ribs, was freely movable on bimanual palpation, and responded conspicuously to the movements of the diaphragm. It was dull to percussion, and the dulness was continuous with the ordinary area of splenic percussion, while above and to the right it merged into the hepatic dulness. This tumour certainly appeared to be an enlarged spleen, yet post mortem examination showed it actually to be the left lobe of the liver, greatly enlarged by the formation in it of numerous malignant masses, doubtless secondary to the intrathoracic tumour; the right lobe of the liver was strictly normal. In this case no confusion in the essential diagnosis resulted, but the incident illustrates how malignant disease in the liver may be distributed so as to produce a tumour having clinically the characters of an enlarged spleen. And considering the important part which the condition of the spleen not infrequently plays in the differential diagnosis of diseases of the liver, the clinical importance of such an occurrence is considerable. The conjunction of events just suggested actually occurred in one (Case CIII) of Murchison's cases, where during life "the liver and spleen appeared to be considerably enlarged," yet at the necropsy it was found that the apparent enlargement of the spleen was due to "the left lobe of the liver reaching far down and to the left," both lobes being greatly enlarged and infiltrated with soft cancer. Frerichs, too, records a case of malignant disease of the liver in which the left lobe was so much enlarged "that the false ribs in the left hypochondrium appeared pressed out." And Bright,[10] among his various experiences of abdominal tumours, describes a case in which "in the left hypochondrium just below the margin of the false ribs there was a tumour the size of a large fist . . . that could scarcely have been referred to any other organ but the left lobe of the liver, the spleen, or the stomach." It proved to be a malignant tumour confined entirely to the left lobe of the liver; and in his commentary on the case Dr. Bright remarks that "the complete immunity of the right lobe of the liver tended greatly to obscure the diagnosis." Jenner, also, draws attention to the ease with which an enlarged left lobe may be mistaken for a splenic tumour, but remarks that "whenever the left lobe of the liver is enlarged sufficiently to cause difficulty in diagnosis, the right lobe is also enlarged." (*Op. cit.*) This statement in one respect comes into conflict with some of the above recorded experiences, but it supports the suggestion that a tumour in the left hypochondrium simulating an enlarged spleen may really be an enlarged left lobe of the liver; and it emphasizes the remark that this possibility must be considered before placing the fact of the *quasi*-enlarged spleen against a diagnosis of cancer of the liver.

A second possibility to be borne in mind in connection with the existence of a tumour in the left hypochondrium, as bearing on the diagnosis of cancer of the liver, is the development of a malignant growth in proximity to the spleen. This event was illustrated by a case in which the detailed diagnosis was a matter of considerable difficulty. It is sufficient here to say, that whilst there were ambiguous signs in the right chest, there was little doubt that the disease had commenced below the diaphragm, and that the liver was gravely at fault. On the whole, the discussion seemed to be between abscess and cancer. As against the latter, it was noted that "in the left hypochondrium is a tumour which is regarded with considerable confidence as an enlarged spleen. In size and shape it quite corresponds to a moderate splenic enlargement: it is influenced by the movements of the diaphragm, is freely movable on bimanual palpation, and is dull to percussion." Yet, post mortem examination showed the spleen to be strictly normal in size, while the tumour above described was found to be a malignant growth (cylinder-celled epithelioma) associated with a cancerous ulcer of the stomach. The ulcer was situated 5 in. from the pylorus and close to the great curvature; it had perforated all the layers of the stomach wall, and the perforation led, not into the peritoneal cavity, but into a number of small spaces, containing a creamy fluid, and within a tumour mass which passed upwards and to the left and became adherent to the concave surface and lower end of the spleen. There were numerous cancerous tumours in the liver, and the spleen was of normal size. The case is a very complete demonstration of the possibility of a malignant growth being situated so as to suggest an enlarged spleen and thus to establish in an obscure case of hepatic disease a presumption against a diagnosis of cancer.

Among the cases of hepatic cancer recorded by Frerichs (*Op. cit.*), there are two which may be quoted in connection with the above record. In the first it was noted during life that "on percussion the stomach appears to be small and the dulness in the left hypochondrium is considerably increased." At the necropsy it was found that "the spleen is connected anteriorly and inferiorly to the ribs by a hard, dense, cancerous tumour, which is opposed to the concave surface of the organ over half its extent; at one place the tumour passes into the parenchyma of the spleen, but everywhere else it can be separated." The spleen was not enlarged. In the second case, no clinical suspicion of splenic enlargement existed, but there was in the left epigastrium and hypochondrium a tumour which was proved to be a malignant growth, and which, as in the case above related, contained a cavity which communicated through an ulcerated opening with the interior of the stomach.

The three cases taken together illustrate some of the difficulties liable to be encountered in the physical diagnosis of hepatic disease. They show, what indeed is a not infrequent experience, that in secondary cancer of the liver there may be little or nothing to direct attention to the existence of a primary tumour even when this exists in, and has ulcerated through, the walls of the stomach; also, that a malignant growth developing directly from the gastric lesion may present itself with such characters as to suggest in the strongest possible manner the presence of an enlarged spleen. And this last fact may tend to confuse the diagnosis in view of the generally received doctrine that splenic enlargement is not to be expected with cancer of the liver.

One other case may here be recorded as illustrative of the circumstances in which an enlargement of the spleen may be simulated. The patient was a woman of middle age who for some months had been losing flesh and strength. There was possibly some extension of the normal hepatic limits but no distinctive physical evidence of malignant disease in the liver. In the left hypochondrium was a considerable tumour, which, so far as its physical facts went, appeared to be an enlarged spleen. It was, however, a large malignant mass growing from the cardiac end of the stomach, and there was no evidence of malignant disease either in the liver or the spleen. The case may reasonably be quoted as a demonstration of the completeness with which a malignant tumour of the stomach may simulate an enlargement of the spleen.

A further personal experience bearing on the present question was provided by a patient in whom there were physical evidences of hepatic enlargement with, in the left hypochondrium, an area of dull percussion and a sense of undue resistance, but without any definite tumour. The case proved to be one of multiple hepatic abscess, and the physical signs in the left hypochondrium were found to depend upon a condition of peritoneal thickening with adhesions in the neighbourhood of the splenic flexure, and the result of old-standing dysenteric ulceration of the colon.

The statement that enlargement of the spleen is not to be expected with cancer of the liver may then be accepted as undoubtedly the rule, but it is necessary to remember that with cancer of the liver there is occasionally also cancerous enlargement of the spleen; and further, that malignant disease in the left lobe of the liver, a tumour of the stomach, and a cancerous growth originating in connection with a malignant ulcer which has penetrated the gastric wall, have, each one of them, been known to present the clinical characters usually associated with an enlarged spleen. Each of these possibilities must therefore be taken into consideration before the existence of a tumour in the left hypochondrium can be pressed in opposition to a suggested diagnosis of cancer of the liver.

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ON THE EYE SYMPTOMS OF LOCOMOTOR ATAXIA WITH A CLINICAL RECORD OF THIRTY CASES.

It is now generally recognized that the disease known as locomotor ataxia may include among its clinical manifestations symptoms other than those which depend on pathological changes in the spinal cord. A number of these are associated with the functions of the eyeballs. Thus, the Argyll Robertson pupil is universally admitted as valuable confirmatory evidence of a diagnosis of locomotor ataxia; ocular paralyses, if less frequent, are certainly not less significant; and optic nerve atrophy is so well known in connection with the disease that its occurrence in any individual case would hardly call for comment.

A further step forward in our knowledge of the clinical possibilities of locomotor ataxia has been a recognition of the truth that the ocular disturbances may precede the evidences of a spinal lesion. This advance necessarily means that the occurrence of any one of the ocular events above mentioned, must, unless otherwise explained, generate the suspicion that the case may in its later events display the phenomena known to depend upon sclerosis of the posterior columns of the spinal cord.

It is difficult to collect the evidence necessary to show in what proportion of cases this suspicion is justified by the event, as the interval between the ocular disturbances and the manifestation of spinal symptoms may be a long one. In the case of optic atrophy it may, according to Gowers, extend even to twenty years. Hence it can only be in very exceptional instances that one and the same physician will have the opportunity of observing any considerable number of these cases through all the stages of their progress. Yet, if true, it is of manifest importance, for the sake both of exact knowledge and of accurate prognosis, that it should be clearly recognized that an optic nerve atrophy, an ocular paralysis, or a loss of the pupil light-reflex, unless capable of other explanation, belongs in all probability to the order of events incident to locomotor ataxia, and that any one of these may well be the introduction to a more widespread manifestation of the disease.

For the reason stated above, the collection of complete histories necessary to afford actual demonstration of the truth of this proposition, is difficult; and all the more so, as there is reason to believe that in those cases in which the early stress of the disease falls upon the nervous apparatus of the eyeball, the spinal symptoms are apt to be slight in degree as well as delayed in development. This is, perhaps, particularly the case when the ocular disturbance takes the form of optic nerve atrophy. "In a large number of such cases," says Gowers, "ataxy never comes on, the spinal malady becoming stationary when the nerve suffers."

Of course, in a given case of optic nerve atrophy without spinal symptoms the question may fairly be raised whether it is right to place such a case in the locomotor ataxy group. All that can be said in reply is (1) that from cases of optic atrophy pure and simple one passes, by an unbroken series of steps, through cases with more and more distinct evidence of locomotor ataxia, to, at the end of the series, optic atrophy in association with characteristic ataxic symptoms; and (2) that, as already stated, a simple case of optic atrophy may remain unchanged for years, and yet in the end may display undoubted evidence of the development of a spinal lesion. But if optic nerve atrophy may be the first symptom of the disease, if the occurrence of spinal symptoms may follow it after an interval of years, and if, again, the atrophy may remain without the development at any time of ataxia, it is not unreasonable to presume that the same three propositions may apply both to the Argyll Robertson pupil, and to an ocular paralysis, in relation to the development of the spinal evidences of locomotor ataxia. The collection of evidence to support this suggestion is even more difficult than in the case of optic nerve atrophy. The latter condition must, ere long, compel the patient to seek medical advice, and thus the opportunity for a complete investigation of the state of his nervous apparatus is afforded at a relatively early date. But an Argyll Robertson pupil may exist, and presumably for years, without any inconvenience to the patient. Such a patient, therefore, will not consult his medical adviser until spinal or other symptoms display themselves, and hence the precedence of the pupillary condition cannot be determined. In the case of an ocular paralysis, assistance is, no doubt, usually invoked promptly. But such an occurrence is open to various etiological explanations, for example, rheumatism, cold, etc., and these are not easily excluded. Hence an ocular paralysis is much less precise in its significance than either a bilateral optic atrophy or the Argyll Robertson pupil. Only therefore by the collection of observations extending over a long term of years, can actual demonstration of the relation of the ocular disturbances now in question to the occurrence of spinal disease, be established. But, while falling short of the merit of actual demonstration, the presentation of the facts displayed by a number of cases which could only be observed over relatively brief periods, is not without value. Though no one case affords a complete history of all the stages of the disease, the picture presented may none the less be fairly complete, provided the cases are sufficiently numerous, and provided they are seen in different stages of the progress and development of the disease.

It is believed that in the series of cases here submitted the conditions just stated are fulfilled. The conclusions they afford, so far as the present purpose is concerned, are: (1) That an optic nerve atrophy, an ocular paralysis, an Argyll Robertson pupil, may each exist as an isolated symptom for a considerable time, presumably for years;

(2) that any two of these may be associated together, with a correspondingly increased presumption that the diseased process causing them is of the locomotor ataxy order; (3) that any one of the three, or a combination of two, or of all of them, may exist in conjunction with a greater or less degree of evidence of spinal disease; and (4) that occasionally a case which commences with purely ocular symptoms may be seen to develop with comparative rapidity characteristic symptoms of the spinal lesion of locomotor ataxia. The cases, therefore, may be held to justify the view that an optic nerve atrophy, an ocular paralysis, or an Argyll Robertson pupil, not capable of other explanation, must be regarded as affording a definite basis for suspicion in reference to a possible development of spinal disease. On the other hand, it must be admitted that the prognostic indication, so far as spinal disease is concerned, is not an absolute one, for the ocular defect may exist, certainly for many years, and perhaps permanently, without any evidence whatever of involvement of the spinal cord.

The cases here recorded have all been the subject of detailed, and in most instances, of repeated examination, and unless the contrary is stated, it may be taken for granted that the thoracic and abdominal viscera were normal to physical and other methods of examination. In all the cases, too, in which no opposing statement is made, it is to be understood that the visual acuity, the visual fields (both for white and colours), and the fundus oculi were proved to be normal. This last statement, of course, does not apply to cases in which optic atrophy existed. Particular care was taken to be accurate in regard to the condition of the pupils and the knee-jerks. In nearly, if not absolutely in every instance where a departure from the normal is chronicled, the record has been confirmed by more than one observer, and in the case of a deficient knee-jerk the conclusion stated has never been formulated until the conditions insisted on by Gowers, Buzzard and Jendrassik have been fulfilled. With a few exceptions, in which only a single observation was possible, the patients have been watched for months, and in some instances for several years. The cases are arranged in series with a view to show how, from a purely ocular condition, one may pass through gradually accumulating evidence to the same ocular condition in association with characteristic signs of the spinal lesion of locomotor ataxia.

I.—CASES IN WHICH OPTIC NERVE ATROPHY WAS THE PRIMARY OR DOMINATING CONDITION.[1]

(*a*) *Optic Atrophy without other Evidence of Disease.*

CASE 1.—W. T., aged 25. Failure of vision extending over two years, with reduction of visual acuity to the power of counting fingers at 3 ft. Double optic atrophy; pupils medium, with distinct light-response; knee-jerks normal and no evidence of spinal disease; no cerebral symptoms other than one or two attacks of giddiness. Urethritis, but no syphilis.

CASE 2.—F. R., aged 38. Double optic atrophy, with almost complete loss of vision, the defect in sight having been observed for at least eighteen months; pupils dilated and immobile; no evidence of spinal disease, unless possibly some degree of failure of sexual power; no cerebral incidents; no history or evidence of syphilis.

(*b*) *Optic Atrophy with other Ocular Evidence suggestive of Locomotor Ataxia.*

CASE 3.—R. C., aged 39. Double optic atrophy reducing right visual acuity to the power to count fingers at 4 ft., and left to mere perception of light; right pupil dilated and three times the size of the left; neither any light-response, but free movement in convergence; entire absence of symptoms and objective signs of spinal disease; "gleet" twenty years before, no syphilis.

CASE 4.—A. S., aged 25. Double optic atrophy with observed failure of vision for twelve months. V.A. right, "hand movements" only, left, 6/18 part; pupils 2.5 mm., no light-response, but contract in convergence; knee-jerks difficult to obtain, but movement, though possibly wanting in promptness, is normal in extent; no ataxia or other evidence of spinal disease; mother of three healthy children, no miscarriages.

(*c*) *Optic Atrophy with some Evidence of Spinal Disease.*

CASE 5.—W. A., aged 37. Double optic atrophy with reduction of visual acuity to "hand movements"; pupils dilated and immobile; knee-jerks absent, but no other evidence of spinal disease; venereal sore when aged 20; no recognized secondaries; father of four healthy children.

CASE 6.—J. G., aged 35. Failure of sight (six months); optic atrophy gradually increasing while under observation for twelve months; pupils not definitely abnormal; knee-jerks absent throughout, but no further appearance of spinal disturbance; urethritis, but no history of syphilis; father of two healthy children, wife no miscarriages.

CASE 7.—F. L., aged 39. Double optic atrophy reducing visual acuity to 6/24; pupils very small, and with Argyll Robertson phenomenon; subsequent to failure of sight (twelve months) has had shooting pains in thighs and failure in retention power of bladder; knee-jerks distinct; no ataxia or sensory defect in lower limbs; venereal sore twenty years before; no recognized secondary syphilis; wife healthy, seven pregnancies, five miscarriages.

(*d*) *Optic Atrophy with distinctive Evidence of Spinal Disease.*

CASE 8.—C. H., aged 38. Failure of sight (two years) from double optic atrophy; pupils medium, with Argyll Robertson phenomenon; moderate double ptosis, but no ocular paralysis; shooting pains in lower limbs (eight years); knee-jerks absent; considerable ataxia and failure of control over bladder; syphilis at 19 years.[2]

CASE 9.—G. S., aged 40. Pallor of discs and peripheral contraction of visual fields; four months later loss of knee-jerks and gradual development of ataxia; pupils normal throughout; death at the end of twelve months with symptoms of meningitis; syphilis at 25 years.

II.—Cases with Argyll Robertson Phenomenon.

(*a*) *Argyll Robertson Phenomenon without other Evidence of Disease.*

Case 10.—A. L., aged 33, the subject of slight hypermetropic astigmatism. Pupils small, not quite circular, with Argyll Robertson phenomenon; no other ocular defect, and no evidence of a spinal lesion. No history of syphilis.

Case 11.—K. S., aged 43. Pupils rather small, unequal, quite destitute of light-response, though moving freely in convergence; no other ocular defect; no evidence of spinal disease, though left knee-jerk not easily obtained. Unmarried; syphilis seems highly improbable.

Case 12.—G. G., aged 60. Pupils small, with distinct Argyll Robertson phenomenon. Knee-jerks not easily obtained, but not definitely abnormal, and no other evidence of spinal disease. Patient suffers from defective vision, probably due to tobacco poisoning (central scotoma for red); no history or evidence of syphilis.

(*b*) *Argyll Robertson Phenomenon with other Ocular Disturbance suggestive of Locomotor Ataxia.*

Case 13.—D. T., aged 42. Pupils below medium size, destitute of light-response, with free movement in convergence; has for seven days suffered from diplopia, and under observation gradual development of complete paralysis of right external rectus; no other ocular defect. Knee-jerks distinct, and no suggestion of spinal disease; chancre of lip and secondary syphilis nine years before.

(*c*) *Argyll Robertson Pupils with more or less Evidence of Spinal Disease.*

Case 14.—T. F., aged 55. Right pupil 2 mm., left 3 mm., each with Argyll Robertson phenomenon; no other ocular defect except presbyopia. Ten years ago had difficulty in passing urine, and since then occasionally voids it involuntarily, and for eighteen years has been liable to seizures of pain in calves, insteps and heels; knee-jerks normal, and no objective sign of spinal disease. Venereal sore when aged 20, but no secondary symptoms, and father of six healthy children.

Case 15.—H. W., aged 38; is the subject of hypermetropia, 4.5 D. Pupils very small, especially left; neither responds to light but distinct contraction during convergence. Admits recent difficulty in descending stairs, saying he "frequently misses the bottom step," and has suffered from "sciatica" for two years. No objective evidence of spinal disease, and urinary and sexual functions undisturbed. Admits gonorrhœa but denies syphilis. Wife miscarried eight months after marriage; no further pregnancies.

Case 16.—R. S., aged 43; hypermetropic and presbyopic. Pupils small, unequal, not quite circular, and with definite Argyll Robertson phenomenon. Knee-jerks cannot be obtained (confirmed on three different dates), but no other sign or symptom of tabes dorsalis unless "rheumatic pains" in lower limbs for several years. Unmarried; no history of syphilis. Four years ago, after "catching cold," had to have urine withdrawn by a catheter, but no subsequent disturbance of bladder function.

Case 17.—E. W., aged 48. Myosis with Argyll Robertson phenomenon; right ptosis and crossed diplopia (one month), without obvious ocular paresis; absence of right knee-jerk (confirmed on two occasions), and failure in retention power of bladder (six months), but no other evidence of spinal disease; vulval sores and skin eruption six years before.

III.—Cases in which an Ocular Paralysis was the Earliest or Dominating Symptom.

(*a*) *Ocular Paralysis without other Evidence of Disease.*

Case 18.—F. D., aged 37. Dilated and immobile pupils, without any other ocular defect. No evidence of spinal disease; syphilis nine years ago; no change while under observation for three years.

Case 19.—K. K., aged 28. Iridoplegia, double, followed by paralysis of left external rectus, the condition being under observation for nearly a year, but without the discovery of any satisfactory explanation. No evidence of spinal disease. Married, four healthy children, no miscarriage; during one pregnancy very free loss of hair (now grown again), but no other occurrence to suggest syphilis. No family or personal history of gout or rheumatism.

Case 20.—G. H., aged 33. Dilated and immobile pupils, with incomplete ptosis and divergence of eyeball on each side. Present condition of four years' duration, and separated by an interval of three years from venereal sore and skin eruption. No other ocular defect; no evidence of spinal disease, and general health good throughout. No change while under observation for three months.

(*b*) *Ocular Paralysis with other Ocular Evidence suggestive of Locomotor Ataxia.*

Case 21.—E. S., aged 43. Diplopia from paralysis right external rectus, pupils small, each with Argyll Robertson phenomenon; visual acuity 6/12, and small but distinct central scotoma, with some contraction of the peripheral field; knee-jerks distinct and no ataxia or other evidence of spinal disease. Three early miscarriages, no full-time child. No change while under observation for nine months, but on two occasions severe attack of vomiting and abdominal pain extending over several days and without recognized cause (? gastric crises).

Case 22.—A. M., aged 55. In 1887, left ptosis and paralysis of external ocular muscles supplied by third nerve, the pupils being normal, followed by incomplete recovery. In 1897 development of identical conditions on right side, and pupils found to be small and with Argyll Robertson phenomenon; knee-jerks very slight and obtained with difficulty, but no other evidence of spinal disease. No history of syphilis.

Case 23.—H. F., aged 37. Right ptosis with diplopia (seven days) and defective inward excursion of right eyeball; pupils very small, not quite equal, and with Argyll Robertson phenomenon; optic discs pale and marked contraction of visual fields, but normal central vision; knee-jerks scarcely to be obtained, but no other evidence of spinal disease. No history of syphilis. Father of four healthy children. Seen after a month's interval, and then paralysis of all external right ocular muscles supplied by third nerve, and knee-jerks absent.

(*c*) *Ocular Paralysis with more or less Evidence of Spinal Disease.*

Case 24.—G. S., aged 41. Ptosis and complete ophthalmoplegia externa on left side, with dilated and immobile pupils and some degree of right ptosis, these conditions, or some of them, having been present for five years. Knee-jerks distinct, and no ataxic phenomena, but imperfect control over bladder and failure of sexual power during last six months. No admitted syphilis.

Case 25.—J. L., aged 42. Diplopia and drooping left upper eyelid for four years. Ptosis left side, and marked defect of ocular movements in each eye; left pupil dilated and immobile, right small, contracts during convergence, but no light-response; no other ocular defect. Knee-jerk scarcely obtained on either side; no ataxia, but attacks of "twitching pains" in lower limbs, and for some time difficulty in starting the flow of urine. Venereal sore in 1882, and subsequent loss of hair, but no other secondary symptoms. Patient watched for twelve months without appreciable change.

Case 26.—L. D., aged 44. Crossed diplopia (one month) without obvious ocular paralysis, and pupils small with Argyll Robertson phenomenon. Knee-jerks absent; no ataxia to usual tests, but has noticed tendency to stagger in the dark; is troubled with pains in the knees, has difficulty in commencing the act of micturition, and recent marked failure of sexual power; venereal sore at 20 years, and subsequent sore throat, but no skin eruption or loss of hair. Father of three healthy children.

Case 27.—J. H., aged 47. Double vision of two months' duration; similar attack three years ago, with complete recovery. Paralysis of right external rectus; pupils, visual acuity, and visual fields normal. Knee-jerks absent, but no other evidence of spinal disease. Patient the subject of albuminuria, and presents physical evidence of an aneurysm of the ascending aortic arch. Youngest child has marked evidence of inherited syphilis.

Case 28.—W. M., aged 35. Paralysis of left third nerve, without iridoplegia or cycloplegia; pupils normal. Knee-jerks absent, but no ataxia or other evidence of spinal disease. Several venereal sores ten years ago, but no recognized secondary syphilis. There is, however, evidence of a former iritis.

Case 29.—M. C., aged 54. Paralysis of left external rectus, with history of two previous attacks of diplopia during last four years; no other ocular defect unless some imperfect light-response in left pupil; knee-jerks absent, and complaint of "sciatica" for two years, but no other evidence of spinal disease. Albuminuria distinct, and physical signs of hypertrophy of left ventricle. No history of syphilis.

Case 30.—H. Y., aged 30. Homonymous diplopia observed over a period of six months without appreciable ocular paralysis; pupils of medium size, with definite Argyll Robertson phenomenon; knee-jerks absent, and in later months decided ataxia and sensory defects in lower limbs; visual acuity normal to ordinary tests but gradual contraction of visual fields, especially on right side; chancre and secondary syphilis four years before.

There are in these series of cases many features which might reasonably be made the subject of remark, and several of the cases are certainly of great individual interest. But they are here displayed in the above grouping for the purpose of illustrating the clinical order and sequence in which, as a matter of actual experience, the ocular disturbances of locomotor ataxia may manifest themselves in relation to the spinal evidences of this disease. Of course, in those cases in which there exists only a single ocular symptom unaccompanied by any sign of spinal disease, it may be objected that it has yet to be demonstrated that such cases are of the nature of locomotor ataxia. It is doubtless to be desired that such cases should be under exact observation as long as the opportunity for further developments exists —that is, for the entire life of the patient. But to insist upon such a condition is a mere counsel of perfection. One must make reasonable use of such evidence as the brevity of life and the exigencies of practice permit. And the evidence here set forth certainly affords a very strong presumption, to say the least of it, of the truth of the doctrines stated in the earlier paragraphs of this paper. Probably the particular proposition which is most likely to be contested is the one which places the Argyll Robertson pupil, equally with optic nerve atrophy and an ocular paralysis, as a possible first event in the eruption of the phenomena of locomotor ataxia. But on turning to the records it will be found that the facts support this suggestion almost as strongly as they support the corresponding suggestion in reference to optic nerve atrophy and ocular paralyses. Attention in this respect may be particularly given to Case 13. This man complains of a quite recent diplopia, and he has undoubtedly had syphilis; and the pupils show the Argyll Robertson phenomenon. It is in the highest degree probable that had the patient been under observation a week or two earlier the condition of the pupils would have been the sole existing ocular abnormality. Yet in view of the development of an ocular paralysis, it cannot be doubted that whether the patient develops spinal symptoms or not, his nervous system is the site of diseased processes of the locomotor ataxia order. When to these facts there are added, as in Cases 14 to 17, illustrations of the various forms and degrees of evidence of spinal disease that may be associated with the Argyll Robertson pupil, it seems impossible to resist the conclusions (1) that the condition of the pupil so named may be the first evidence of locomotor ataxia; (2) that it may precede by varying intervals other evidences of the disease; and (3) that at least very probably, and in a certain number of cases, the symptomatology of the disease may be permanently restricted to this one event.

In some examples of its spinal form locomotor ataxia is undoubtedly an extremely chronic disease, and with few and imperfectly developed symptoms. Hence it is not unnatural to expect that similar limitations may obtain in the ocular manifestations of the disease. That evidences of grave nervous disease may be limited to the pupil is well seen in Case 18, where a syphilitic patient was under observation for three years without the discovery of any abnormality other than paresis of each sphincter iridis. There is certainly no obvious reason why a similar restriction should not determine the Argyll Robertson pupil as a purely isolated phenomenon, with, it must be added, the same unfortunate possibilities that are undoubtedly attached to the patient whose case has just been quoted. The conclusions above adopted in reference to the Argyll Robertson pupil are applicable *mutatis mutandis* to optic nerve atrophy and to ocular paralyses, as is abundantly demonstrated in the corresponding series of cases recorded with this paper.

NOTES.

[1] In all cases the ophthalmoscopic appearances were those of primary optic atrophy.

[2] This statement, both here and in other cases, must be taken as implying that there is a quite undoubted history of a primary sore followed by secondary symptoms; when the evidence is less conclusive, the record carries a mere statement of the facts.

ON INEQUALITY OF THE PUPILS AND IRIDOPLEGIA.

WHEN a severely critical comparison is made between opposite sides of the body it will frequently be found that there are defects in symmetry more marked than would have been detected by an ordinary or conventional observation. Occasionally these defects reach a high grade, and they then attract more or less general attention. These more gross contrasts may be worthy of classification as developmental errors, but the slighter and more common degrees of asymmetry exist, not only without appreciable utilitarian or æsthetic prejudice to their possessors, but also apart from the suspicion of any definitely pathological causation. If these statements are true—and they can readily be verified—in reference to the formation of the trunk and limbs, the shape of the head, and the contour of the opposite sides of the face, it is not a matter for surprise to find that they apply in some measure to the size of the pupils. On the exact percentage of those in whom a distinct difference in this respect can be recognized various observers have come to different conclusions, but all agree that in a certain number of persons free from all evidence of disease one pupil is larger than the other. The numbers have been placed as low as 1 per cent. on the one hand, and as high as 10 per cent. on the other; the sexes appear to be affected in equal degree; and it is generally allowed that the left pupil in these cases of inequality is commonly the larger of the two. In some instances the inequality has been noted in association with a difference in the colour of the two irides, and it has been suggested that a less pigmented iris will permit the penetration through its substance of more light than will be allowed by one containing a larger amount of pigment, and that thus the corresponding pupil will receive a stronger reflex stimulus and will, therefore, be the smaller of the two. Such a suggestion is hardly in accord with the known facts of anatomy and physiology. For, the arrangement of the visual nerve tracts is such that light falling on either retina stimulates the reflex pupillary centre for each eye, and hence, even though the amount of light admitted to the two eyes may differ, the total stimulating force acting on each reflex centre is approximately the same. Further, and as a matter of fact, difference in the size of the two pupils, in the circumstances now under discussion, may exist apart from a difference in the tint of the irides; and, on the other hand, the irides may be differently pigmented, and yet the pupils be equal in size.

Upon what element or factor these peculiarities depend can be at present only a matter of conjecture, and it is not impossible that the exact localization of the minute flaws in the process of development responsible for them, and for other minor errors of symmetry, may long or even permanently defy embryological analysis. But to the physician, though the question of etiology may here make little appeal, the undoubted experience that occasionally a person may be found in whom, and merely as a personal peculiarity, the pupils are unequal, is of great moment. For it is a matter of common clinical experience that such a condition may be a piece of evidence of dominating importance in an individual diagnosis and prognosis. Hence, when a difference in the size of the pupils is discovered, it becomes necessary to recognize that the fact may or may not have a serious significance. And the selection of the alternative must largely be determined by the answer to the question, Is the inequality a new development? To secure a confident answer to this question is, however, not always an easy task. Persons whose pupils have always been unequal may live for years without discovering the condition until some symptoms of ill-health or other accidental circumstance causes a more or less critical self-examination. Hence, the observation may be presented as a new development when it is really of very old standing.

A practical lesson that arises from these considerations is that it would be well were the pupils more commonly, and as a matter of routine, included within the scope of the ordinary clinical examination. The information obtained in this way is often of the highest significance, and, whether in its positive or negative aspects, is at least as valuable, and certainly not less precise, than that afforded by the time-honoured examination of the pulse and inspection of the tongue. One result, were the practice now advocated commonly adopted, would be, that what may be called inequality of the pupils natural to the individual, would often be discovered in early life, and provided the condition were made known to the patient or his friends, the risk of an anxious interpretation being applied to it in later life would be avoided. The suggestion or view that ocular conditions or symptoms are mainly the concern of the ophthalmic surgeon is far too generally adopted. These have, on the contrary, the closest possible relation to the work of the physician and the family practitioner, and ought to be made the subject of routine observation.

The question may now be asked whether inequality of the pupils may occur as the first event in the evolution of any form of organic disease. It is, of course, well known that such a condition is an important and not infrequent symptom in such diseases as tabes dorsalis and general paralysis of the insane. But here there are usually other defects, such as bilateral loss of the light-response, in addition to various evidences in other parts of the body, not to speak of mental abnormalities. And certainly, apart from such clinical existences, mere inequality of the pupils would be but a slender foundation for the diagnosis of either of the serious diseases just named. The point now to be dealt with is rather that presented by a conspicuous difference in the size of the pupils regarded as a new fact, and in a person otherwise free from all evidence of disease. First, as already stated, there is the possibility that what is regarded as new, may be merely the recent discovery of something really old. Supposing, however, that the larger pupil is not only dilated, but also that it fails to react to light when this is applied both to the same and to the opposite eye—that is, both the direct and consensual light-responses are absent—it is obvious then that there is not only a dilatation of the pupil, but also a paralysis of the sphincter iridis. In such circumstances, unless there is positive evidence in support of such a view, it will be difficult to believe that the condition is merely an individual peculiarity. In short, while inequality of the pupils with persisting light-response in each, is, unless the contrary can be shown, probably a condition natural to the individual, a pupil both dilated and immobile establishes a presumption in the opposite direction, and unless such a combination can be proved to have existed from early life it must be condemned as definitely pathological. With unilateral disease of, or injury to, the optic nerve or retina, some disturbance of the corresponding pupil is, perhaps, to be expected; but here vision will be defective, the ophthalmoscope will show changes in the fundus, and even if the direct light-response is absent, the pupil will contract when light is allowed to fall on the opposite retina. In the absence of all these conditions the interpretation of a dilated and immobile pupil calls for further inquiry.

Perhaps the first suggestion which arises is that by accident or design the patient has been in contact with some preparation of belladonna or atropine. If this is the case, there will be not only dilatation and immobility of the pupil, but also paralysis of the accommodation; and to read small type at a distance of 12 in. or less will be impossible. Further, in the course of ten days or so the paralysis in both respects will disappear, that is, unless the application is renewed.

Supposing, however, that the dilated and immobile pupil can be shown to be a new event, and, further, that the influence of a mydriatic can be excluded, what diagnostic and prognostic values are to be attached to it? In answer to this it must be recognized that the condition is a definite paralysis, and that its existence therefore throws doubt on the integrity of the nervous system. An extra-ocular paralysis, it will be allowed, has this significance, and it is reasonable to apply the same conclusion to a paralysis of the sphincter iridis. Further, there is distinct clinical evidence that a paralysis of very limited distribution, and sometimes also of limited duration—an iridoplegia, a cycloplegia, a diplopia, for example—may be the first event in the clinical evolution of serious degenerative nervous disease, and, what here has specially to be emphasized, that such paralysis may long precede any developments of more precise diagnostic significance. It therefore follows that any one of these events—a dilated and immobile pupil, for example—must excite some anxiety, and this more especially when the patient is one who must be regarded as predisposed to organic nervous disease on account of syphilitic infection or other agency. On the contrary, when these latter influences can be excluded, while there must be some degree of fear in reference to the future both of the individual lesion and the nervous apparatus generally, it is not impossible that an iridoplegia may have a relatively benign explanation. Such indefinite agents as "cold" and "rheumatism" would appear to be responsible for many facial palsies, and also for some paralyses of the extra-ocular muscles. Their aid may perhaps also be invoked in occasional instances of dilated and immobile pupil, though such an explanation must be advanced with considerable mental reservation. Altogether, therefore, inequality of the pupils and an iridoplegia are in much the same position as that occupied by other varieties of ocular paralysis. Each may possibly be a congenital peculiarity; each may, perchance, signify progressive organic disease; and each may occasionally be due to some uncertain and comparatively unimportant element, named, more or less accurately, "cold" or "rheumatism." In addition, the possibility of atropine, cocaine, or other mydriatic must not be forgotten.

The discussion of inequality of the pupils can hardly be closed without a reference to this condition as associated with lesions within the thorax. An aneurism is perhaps the best known of these, but tumour, phthisis pulmonalis, pleurisy, and pleural effusion, have each, also, been held responsible for pupillary inequality. The usual explanation offered is that these conditions, one and all, produce the inequality by disturbing the functions of the sympathetic nerve, in the upper dorsal and cervical part of which the fibres supplying the dilator pupillæ are to be found. This explanation certainly does not meet all cases, and perhaps it is safest to fall back on two elementary physiological laws, namely, first, that peripheral irritations tend to dilate the pupil, and secondly, that all reflex disturbances manifest themselves first on the side of the body to which the irritation is applied.

ON PERIPHERAL NEURITIS AND RETINAL CHANGES IN DIABETES MELLITUS.

AMONG the relatively infrequent clinical phenomena occasionally associated with the presence of grape sugar in the urine are (1) symptoms dependent on changes in the nervous system, and (2) evidences of disturbance in the fundus oculi. The attempt to explain the facts of diabetes as consequences of vascular and perivascular changes in certain parts of the brain, has, it is true, not met with any considerable degree of success. But the clinician, not less than the pathologist, is alive to the degenerations to which the peripheral nerves are liable in diabetes mellitus, and which, equally with the changes in the central nervous system, are doubtless due to the impaired nutritive value of the blood.

The loss of the knee-jerks and the occurrence of neuralgias may be mentioned as fairly frequent results of (probable) peripheral neuritis of diabetic causation. Less commonly the evidences of nervous disturbance are of a still more conspicuous character, and though some of these—*e.g.*, hemiplegia—strongly suggest a central cause, others depend, almost certainly, on the peripheral neuritis which has been proved to occur in diabetes by actual histological examination. Among this latter group are certain ataxic phenomena and the paralysis of individual muscles.

Pavy,[1] in 1887, noted the existence of sensory disturbances in a certain proportion of diabetic patients; he also described the occurrence of ataxia in walking and without any disturbance of equilibrium as in

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[1] *Medical News*, Philadelphia, 1887, p. 360.

Romberg's test. This observation has been confirmed by Buzzard,[1] by Davies Pryce,[2] and by Fraser and Bruce[3]; and both Pryce and Bruce were able to demonstrate degenerative changes in the anterior and posterior tibial nerves. The combination of ataxia with loss of the knee-jerks forms a clinical picture which it is necessary to avoid confusing with that of tabes dorsalis, and the confusion is all the more easy if, as stated by Saundby,[4] the ataxic symptoms of diabetes may be associated with loss of the pupillary light-reflex. And yet again, the similarity of the two conditions may be heightened by the development in diabetes, as in tabes, of perforating ulcer of the foot; this occurred in one of Buzzard's cases and in two of those reported by Pryce. The last-mentioned observer has also described an erythematous œdema which he regards as dependent on peripheral neuritis. And, as another example of neuritis, Raymond[5] reports the occurrence of herpes in association with facial paralysis in a diabetic patient.

With regard to muscular paralyses it seems necessary to distinguish between hemiplegic and other widely-spread palsies, and palsies of strictly limited distribution. The former must almost necessarily depend on a central nervous lesion, and though cases are recorded in which an opposite view is expressed, none of them seem free from a definite suspicion of hæmorrhage or other intracranial lesion. When the paralysis affects merely one limb or part of a limb there may be more room for doubt as to the site of the lesion, but as such paralyses are, according to Ross,[6] "often incomplete and may sometimes be so transitory as to pass off in a few hours," the balance of probability would appear to be in favour of their central causation. And this seems all the more likely when it is known that the brain tissue on post mortem examination sometimes shows small cavities, as from atrophy of its tissue, while enlargement of the perivascular spaces, as described by Dickinson[1], is not uncommon. When the distribution of the paralysis indicates that only a single nerve is involved, the hypothesis of a peripheral neuritis is of course tenable, if not actually inevitable. This is the position in the case reported below, for not only is the external rectus the sole muscle paralysed, but the paralysis is absolute—and each of these considerations points decidedly to a peripheral neuritis as the cause of the paralysis. Anderson[2] has reported a case in which some of the branches of the third nerve were alone attacked, and most of the recent writers on the subject of diabetes include diplopia, strabismus, mydriasis, and ptosis, among the occasional complications of the disease. It would seem possible also that single nerves other than those supplying the ocular muscles may be selected for local inflammatory attacks, as Althaus[3] has recorded a case in which severe pain in the area of the circumflex nerve was followed by atrophy and paralysis of the deltoid, the patient being the subject of glycosuria and all other possible causes of the neuritis being apparently excluded. Confirmation of this view is provided by Bruce's case already quoted, where there existed a central scotoma in one visual field and the post mortem examination revealed a band of degenerated fibres throughout the course of the corresponding optic nerve. It is at least interesting, and possibly very nearly related to the present subject, to note that a paralysis of the sixth nerve has been observed in a case of diabetes insipidus.[4]

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[1] *British Medical Journal*, Vol. i, 1890, p. 1419.
[2] *The Lancet*, July 2, 1887, p. 11; and *Brain*, 1893, p. 416.
[3] *British Medical Journal*, Vol. i, 1895, p. 1149.
[4] "A System of Medicine." Sir Clifford Allbutt. Vol. iii, p. 218.
[5] *Bulletin Médical*, 1897, xi, p. 286.
[6] "The Diseases of the Nervous System." By James Ross, M.D. Second Edition, p. 934.

The occurrence of changes in the fundus oculi in diabetes is now well established. These at first were attributed to albuminuria accompanying the condition of glycosuria, but no observer now questions their possible development in uncomplicated cases of diabetes. Many careful attempts have been made to establish a line of distinction between the ophthalmoscopic appearances of albuminuric retinitis on the one hand, and of diabetic retinitis on the other. It still, however, remains true that while certain features may be stated which afford a presumption, more or less strong, in one direction or the other, individual cases occur in which the appearances are ambiguous, or in which the presumption is not justified by the event; indeed, the published descriptions of various authorities are by no means agreed as to what are the features which are to be insisted on as "typical." Most authorities, however, allude to the grouping in the retina of the whitish spots which may be seen in each of the two diseases. The well-known stellate arrangement of these in lines radiating from the macula is generally regarded as indicative of albuminuric retinitis, and one of the interesting features of the case here reported is the existence of a similar though incomplete arrangement in a case of diabetes. Saundby[1] goes so far as to say that the patches of diabetic retinitis are "never fan-shaped"; according to Gowers[2] the star around the macula is seldom seen in diabetes, "although rarely there may be a perimacular circle of spots"; while Stephen Mackenzie[3] describes the grouping of the white spots into constellations, which are found "grouped round the macula but are not limited to that situation." The point is, of course, of little practical importance, for not even the most devoted adherent of the art of "spotting" would propose in any given case to conclude his diagnosis without an examination of the urine. Still, accuracy is desirable, and the actual occurrence of a suggestive, if incomplete, stellate arrangement in a diabetic case is at least a warning against undue confidence in making distinctions.

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[1] "Diseases of the Kidney and Urinary Derangements." Part i, p. 934.

[2] *Ophthalmic Review*, Vol. viii, p. 38.

[3] *The Lancet*, March 1, 1890, p. 455.

[4] J. H. Lloyd, *Journal of Nervous and Mental Diseases*, Vol. xxiii, p. 778.

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[1] *Medical Annual*, 1897, p. 193.

[2] "Medical Ophthalmoscopy." Third Edition, p. 229.

[3] *Royal London Ophthalmic Hospital Reports*, Vol. ix, p. 134. See also Nettleship and Stephen Mackenzie: *Transactions of the Ophthalmological Society*, Vol. vi, pp. 331-3.

Another feature in the ophthalmoscopic examination of the case now recorded is the development, to a comparatively advanced extent, of retinal changes in one eye, with a normal fundus in the other eye. Similarly, cases of uniocular albuminuric retinitis have been reported, and in one of these it is said that only one kidney was present—an instance of pathological neatness almost provoking suspicion. The limitation of retinitis to one eye in a patient with diabetes appears to be very rare. In a tabulated statement of recorded cases, made in 1895 by Oscar Dodd,[1] there is no example of such a limitation. Several of the cases quoted in this table also show the practical impossibility of insisting upon any absolute distinction between the ophthalmoscopic appearances of albuminuric and diabetic retinitis.

In addition to retinitis and hæmorrhages in the retina it has been observed that these, in diabetes, are apt to be complicated with hæmorrhages in the vitreous; the existence of a central scotoma in the visual field has also been noted. Neither of these conditions existed in the present case. The record (November, 1897) of the case is as follows:—

The patient was a woman, aged 67. She complained of failing sight during the past twelve months; also of double vision, which came on suddenly and had been present for three weeks. There was absolute paralysis of the outward movement of the right eye. Each lens showed numerous peripheral striæ (ordinary senile cataract). In the left fundus were two considerable patches consisting of yellowish-white lustrous spots more or less confluent; each patch had a somewhat triangular or fan-shaped form, the apex being at the macula; and scattered irregularly over the fundus were a number of isolated spots of a similar character. The right fundus appeared to be normal. There was no central scotoma in either visual field. The urine was pale, of specific gravity 1038, and contained a large quantity of sugar; there was no albumin. Physical examination of the thoracic and abdominal viscera was normal. Knee-jerks could not be obtained. The patient had enjoyed fairly good health except that within the last three years she had had two severe attacks of sciatica (each in the left limb), and six months ago she was confined to bed for some time with "weakness of the heart." Her husband died from angina pectoris twelve months ago. She has had nine children, of whom four have died from consumption. There is no history of gout or of rheumatism in the family. She admitted that the daily amount of urine had been excessive for some six or seven years and that during the same time she had suffered from thirst; about four years ago she was told that she had diabetes.

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[1] *Archives of Ophthalmology*, 1895, p. 206.

The patient has been examined on several occasions since the above date. The urine has always had a specific gravity of 1030 or upwards and has contained much sugar; at no time has albumin been present. The paralysis of the right external rectus and the condition of the left fundus remained unchanged until the end of January, 1899; then the paresis disappeared, and later the changes in the left fundus became much more advanced, the right fundus still remaining intact.

The diagnosis of the case is manifestly beyond dispute. The absence of albumin from the urine leaves the diabetic state entirely responsible for the condition of the fundus no less than for the peripheral nerve disturbances. These latter it will be noted include loss of the knee-jerks, paralysis of the right sixth nerve, and two severe attacks of sciatica. Saundby (*Loc. cit.*) has drawn attention to the occurrence of sciatica in diabetic subjects and says that when sciatica is bilateral it is especially suggestive of diabetes.

ON SECONDARY PAROTITIS.

PAROTITIS, apart from mumps, has been described as an incident in association with a considerable number and variety of pathological conditions; and the exact relation of these conditions to inflammation of the parotid has given rise to some diversity of opinion.

One group of the cases in which a secondary parotitis has been observed is affections of the generative organs. Thus, injury of the testicle [1] has been followed by parotitis, and Trousseau [2] records a case of suppressed menstruation in which, at each monthly period, an inflammatory condition of one or other parotid region occurred. Other cases, also, have been observed in which functional disturbance of the salivary gland has coincided with the occurrence of menstruation [3]; and in one case, described by Stephen Paget [4], swelling of the parotids appeared in successive pregnancies, and was the first subjective indication of this condition. Even the introduction of a pessary into the vagina [4] has been followed by parotitis. The development of inflammation of the parotid gland in such circumstances as those just mentioned has been regarded as the converse of what sometimes happens in mumps, in which orchitis is a recognized complication, while female patients are liable, though less frequently, to the development of inflammatory troubles in the breasts [5] or, more doubtfully, in the ovaries [6]. Further, it is to be noted, that, at least in the experience of some surgeons, parotitis is specially likely to occur after operations on the female generative organs [7], and this, too, without any indication of general pyæmia.

In reference to the cause of the association of inflammatory and other disturbances of the generative organs with parotitis, various suggestions have been made. Sir Jonathan Hutchinson [8] finds an explanation difficult, "unless it be that the parotid gland and testis, when inflamed, develop elements which are mutually infective." Bumm [9], who believes there is a special relation between ovariotomy and parotitis, suggests that as a result of nerve irritation following the operation, the secretion of saliva is suspended by reflex influences, and that micro-organisms then enter the parotid gland through Stenson's canal.

A possible further illustration of the pathological relations between the generative organs and the parotid gland is afforded by the occasional occurrence of orchitis [10], and, though in still more rare instances, of parotitis [11], as a result of gout.

Again, parotitis is a recognized though uncommon complication of some of the specific fevers. Murchison [12] found it most frequently in typhus, and regarded its occurrence in this disease as establishing a connecting link between typhus and Oriental plague. He also described parotitis in relapsing fever and in enteric fever; and in the experience of other authorities it appears to have been most frequently observed in the last-mentioned. Gee [13] regards the inflammation of the parotid in these cases as a result of dryness of the mouth, the irritation extending from the buccal mucous membrane along Stenson's duct to the substance of the gland. This view has been supported by other observers, and it has been maintained that from the very earliest appearance of the symptoms of parotitis, pressure over the duct of the parotid will cause the expulsion of a drop of pus from the duct-orifice [14]. As the result of histological examination of the duct and gland in several fatal cases, Hanau [15], also, concluded that the parotitis was the result of infection from the buccal cavity. It is interesting, in this connection, to note the occurrence of recurring attacks of parotitis, or at least of swelling of the parotids, in cases of xerostomia or dry mouth. Several examples of this association have been described [16], and Sir Jonathan

Hutchinson, who has recorded a number of such cases, regards the two conditions as resulting from some nervous disturbance. In the majority of the cases of this character the xerostomia has been a very chronic condition, and has developed without apparent cause; but Hutchinson [17] also reports two cases in which dryness of the mouth, with painful swelling of the parotids, appeared on the third day after an injury (which subsequently proved fatal) to the abdomen. In each case the swelling disappeared in the course of a few days and without suppuration. It is very difficult, in these circumstances, to regard the parotitis as pyæmic in character; and further, as there was in each of the patients, and as immediate results of the accident, copious sweating, followed by dryness of the skin as well as of the buccal mucous membrane, and the passage of an excessive quantity of urine, it seems a fair inference that the violence to the abdomen produced reflexly through the nervous system disturbance of the function of various glands, including the salivary glands. It is, of course, still open to argument that the parotitis was not a direct but an indirect result of this disturbance, and that the cessation of the salivary secretion either gave an opportunity to micro-organisms or other irritants to reach the gland through the unoccupied Stenson's duct, or, in some other way, rendered the gland susceptible to the influence of toxic agents.

The relation of parotitis to pelvic and abdominal lesions has received considerable attention from Stephen Paget, who has come to the conclusion that cases of parotitis associated with such lesions form a special and distinctive group. Paget questions the pyæmic origin of the inflammation. He has collected 101 cases [18] of parotitis following abnormal conditions in the pelvis or abdomen, and he points out that in the great majority of these the parotitis, as an inflammatory event, was an isolated one; that in a considerable number of the cases there was no suppuration; that the attack was, as a rule, unattended by rigors or by any decided rise of temperature; and that the issue of the case was in no practical sense prejudiced by the parotitis. These facts he considers conclusive against the suggestion that the inflammation of the parotid was due to any "ordinary form" of pyæmia. Paget also rejects the suggestion that the irritation travels from the buccal cavity backwards along the parotid duct, and instances, in support of this view, the experience that it is quite exceptional in these cases to find the *socia parotidis* specially affected, or attacked at an earlier date than the main body of the gland. The view that Paget advocates is that there is some special connection, through the nervous system, between the abdominal and pelvic viscera and the salivary glands, and that lesions of the viscera lead reflexly to a congestion of the gland, which may proceed to inflammation, and even to suppuration. And in support of his position he refers to the recognized influence which morbid conditions of the stomach exercise upon salivary secretion; to the experiments (upon dogs) in which inhibition of the secretion of saliva is produced when a coil of intestine is withdrawn from the abdomen, the secretion returning when the intestine is replaced; and to the occasional occurrence of salivation during pregnancy.

Besides Paget's collected cases, other examples of the occurrence of parotitis in connection with abdominal and pelvic lesions have been put on record. Christopher Martin [19], after an experience including a thousand cases of abdominal section, speaks of it as a "curious complication," which "usually occurs during the second or third week, and may nearly always be traced to a septic cause." It may be noted, however, that other surgeons have observed the development of parotitis at a much earlier date. Thus, in Mr. Jalland's case, already alluded to, the parotid swelling appeared on the third day after operation. Fagge [20] describes parotitis in a case of intestinal obstruction due to cancer of the sigmoid flexure; Taylor [21] observed it after an operation for imperforate anus; Sir Hector Cameron [22] after abdominal section; Middleton [23] in a case of fæcal tumour and also in a case of pernicious anæmia [24]; Grisolle [25], from experience in the Vienna Hospital, notes it as occurring on five occasions in a total of upwards of 5,000 cases of pneumonia; Gowers [26] describes parotitis as an event in a fatal case of peripheral neuritis following a stab wound under the left clavicle, the wound "healing well"; Fagge mentions it as an occasional complication of influenza [27] and of yellow fever [28]; two cases have been noted in which parotitis complicated an attack of illness regarded as of the nature of acute rheumatism [29]; and Gee [13] records the occurrence of parotitis in a fatal case of chorea accompanied by a very dry state of the mouth and tongue.

The four cases briefly recorded below have all come under my observation in hospital practice. The first was regarded, without much doubt, as a case of gastric ulcer, though the rapidity of the convalescence may possibly introduce a question as to the accuracy of the diagnosis. In the second case, the slight development of gastric symptoms other than the hæmatemesis, and the prompt improvement of the patient, leave it an open question whether the case is to be marked as one of "latent" gastric ulcer, or as belonging to the group, described by Trousseau and others, in which hæmatemesis occurs without any ulceration of the gastric mucous membrane. In neither of these patients was there any fact, either in the history or the physical diagnosis, to suggest cirrhosis of the liver or other cause of portal obstruction.

The occurrence of parotitis in cases of gastric ulcer and hæmatemesis must, I think, be a most unusual event.[1] In Mr. Paget's [18] list of 101 cases of secondary parotitis, eighteen are classified as following disease or injury of the alimentary canal, and in only one of these was there any question of the existence of a gastric ulcer. Even in this case the diagnosis was doubtful, and as the patient was at the time suffering from "two small bedsores," it by no means follows that the parotitis was consequent upon the gastric ulceration, allowing that this was actually present.

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[1] See, however, Discussion at Royal Medical and Chirurgical Society, *Transactions*, 1905.

The third case here reported illustrates the occurrence of parotitis in lobar pneumonia; it provides an exception to the experience of Grisolle [25], who found this complication a serious one, as in his cases the inflammation usually terminated in suppuration or gangrene. In the fourth case the parotitis may fairly be regarded as following the appearance of purulent matter in the peritoneal cavity.

Case 1.—Jean J., aged 42, unmarried. The history gives a record of epigastric pain, with almost constant vomiting, of three weeks duration, and, on the morning of admission to the hospital, the occurrence of a free hæmatemesis. Previous health good, except for mild attacks of gastric disturbance at intervals during the last three years. Under rest in bed, the use of bismuth subnitrate, and the absence of solid food, the stomach symptoms soon subsided, but a fortnight after admission pain was complained of under the right ear; the right parotid region rapidly became swollen and extremely tender, the temperature rising to 101° F. In the course of forty-eight hours, however, the temperature fell to normal, and the swelling gradually disappeared without suppuration.

Case 2.—Janet L., aged 26, unmarried, admitted to hospital on account of a severe hæmatemesis extending over twenty hours. The hæmatemesis was repeated three days after admission, and was followed by melæna. Four days after the second hæmorrhage evidences of right parotitis appeared, the temperature running up to 101.8° F. Under treatment, the local condition improved and fever subsided, but after a few days' interval pain and swelling returned, and the temperature reached 102.2° F. Fluctuation could now be detected, and a large quantity of pus was evacuated on incision. The patient had always enjoyed good health, but had, from childhood, been apt to vomit her food. For a week before the hæmorrhage she had experienced some pain after food, but this had not been severe, and there had not been at the time any vomiting. Physical examination negative, except for undue pulsation in the abdominal aorta. After relief of the parotitis by incision, the gastric symptoms subsided, and the patient made a rapid recovery.

Case 3.—Mary I., aged 17, admitted with pneumonia of left lower lobe; crisis distinct on ninth day. During the next ten days patient remained well, except for a slight rise of temperature, which soon passed away and for which no explanation could be found. At the end of this time she complained of pain at the left angle of the jaw and of headache, and the temperature rose to 103.6° F.; no rigors; fulness and tenderness were detected over the left parotid gland. In the course of three days the temperature fell to normal, and all the other symptoms disappeared; subsequent convalescence uninterrupted.

Case 4.—Thomas B., aged 46. This patient was the subject of aortic disease, with amyloid changes in the viscera, and evidences of syphilis in the liver. In the latter part of his illness ascites became troublesome, and six days before death the abdomen was tapped. Two days after this operation both parotid glands became much swollen and very painful, and remained so until death. Post mortem examination revealed the conditions mentioned above; also the presence of fluid with soft yellow fibrinous exudation in the peritoneum, and pus infiltrated through each parotid gland.

It cannot be claimed that these cases contribute any very definite support to any of the various hypotheses suggested to explain secondary parotitis. But it may be observed that in each one of them opportunity existed for the entrance of toxic matter into the blood, and it is, therefore, at least possible, that under certain circumstances the parotid gland may discharge an excretory as distinct from a secretory function, and that, in the act of excreting toxic or other materials, it may be "irritated" into a state of inflammation or even suppuration. That the secretion of saliva may be influenced reflexly through the nervous system there can be no doubt, but it is difficult to believe that inflammatory swelling, or suppuration, can be explained in this way. And it may fairly be questioned whether the swelling of the parotid gland which is known to occur in cases of chronic xerostomia, and in certain other conditions, is of the same nature as the parotid inflammations which follow traumatic or other lesions, such as have been above described, in various parts of the body. If secondary parotitis were due to the passage of irritants from the buccal cavity into Stenson's duct, one would certainly look for its more frequent occurrence; and this especially in view of the large number of cases in which, with febrile phenomena, there is extreme dryness of the buccal mucous membrane.

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[29] *British Medical Journal*, 1885, Vol. ii, p. 14.
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I. OPAQUE NERVE FIBRES EXTENDING OVER A LARGE AREA OF THE FUNDUS INCLUDING THE REGION OF THE MACULA.

II. PATCHES OF OPAQUE NERVE FIBRES SEPARATED FROM THE OPTIC DISC.

THE discovery of opaque nerve fibres in the fundus oculi, although hardly a frequent ophthalmoscopic experience, occurs sufficiently often to merit in ordinary circumstances no more than a mere word of recognition. The percentage of such cases in relation to the total number of eyes examined is doubtless a very small one, but a routine use of the ophthalmoscope extending over any considerable number of patients can scarcely fail to detect an occasional example. And as the condition, with rare exceptions, has no practical effect on vision, the opportunity for its discovery is just as likely to occur to the physician or other practitioner as it is to the ophthalmic surgeon.

As is well known, the most frequent arrangement of opaque nerve fibres in the fundus is in the form of one or more lustrous white patches jutting outwards from the margin of the disc (fig. 30). Possibly, by a very critical scrutiny of the visual field, it can be shown in some cases that there is a corresponding extension of the normal area of the "blind spot," but, this apart, such patches neither produce a subjective sense of impaired vision nor do they qualify the ordinary tests of the visual acuity.

Exceptions to the propositions just stated are so uncommon that the cases here figured appear to be worthy of record. In the first (fig. 31) it will be observed that the opaque nerve fibres are not restricted, as in the more ordinary form (fig. 30), to one or more isolated patches on the disc circumference, but that, on the contrary, they completely encircle the disc. Further, instead, as is the common fashion, of extending over the fundus from the disc margin for a short distance only, say a disc's breadth or less, the opaque fibres here thrust themselves over so large an area that they form by far the most conspicuous feature of the ophthalmoscopic picture. Especially is this unusual extension pronounced to the outer side of the disc. So extreme is the development in this direction that the opaque fibres are continued over and conceal

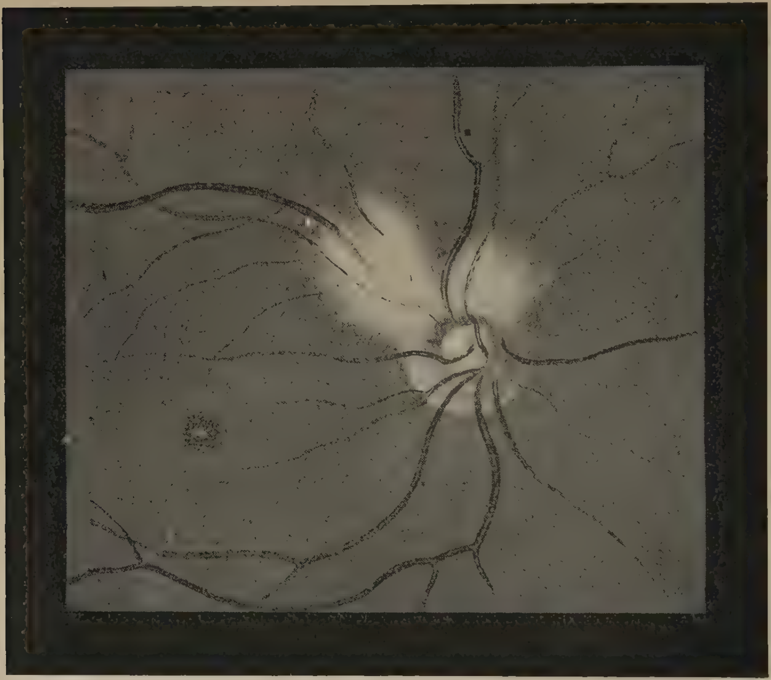
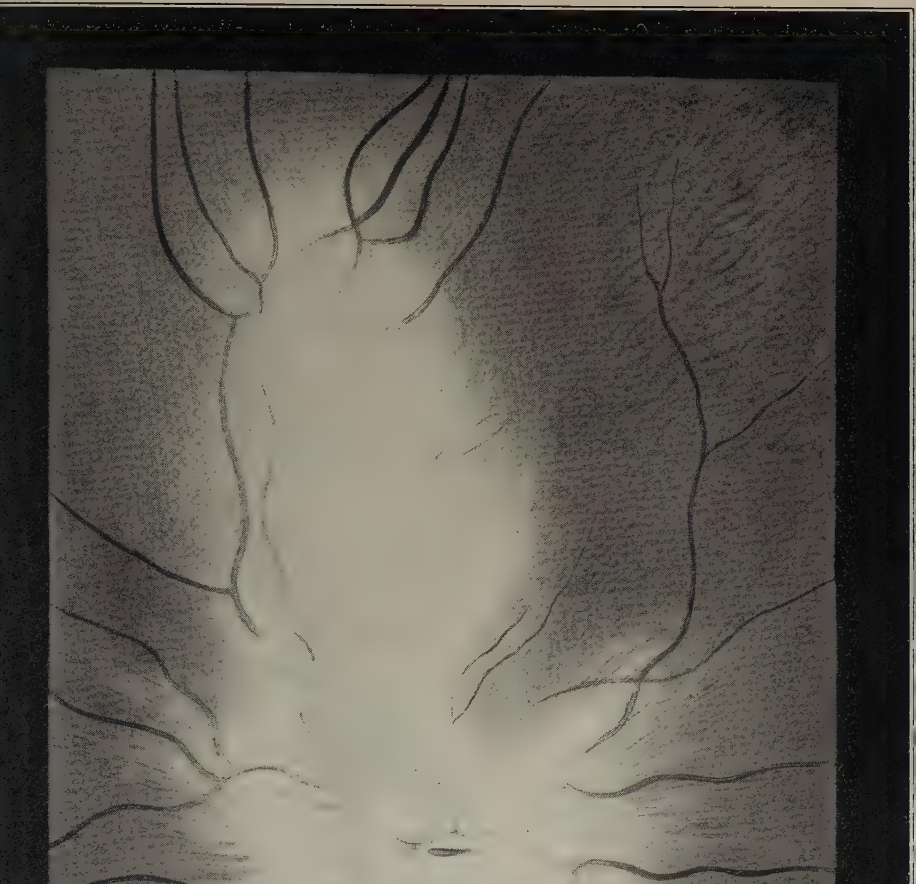


FIG. 30.—Opaque Nerve Fibres in their more usual distribution, that is, in bundles or patches attached to the margin of the Optic Disc.



the site of the macula, and even a considerable portion of the fundus beyond this. It is of interest to observe that in spite of this highly exaggerated provision of a medullated sheath to the nerve fibres of the retina, the disc itself, except for a small part of its lower and outer quadrant, is not involved in the process. Another detail worthy of remark is the absence from the opaque area in the macular region of the fibrillated lustrous appearance which is so characteristic of the familiar small patches attached to the disc margin, and is also a feature of other portions of the present picture. The macular portion of the opaque area has, on the contrary, a "dead" white character and some apparent irregularity of surface. Of its true nature, however, there can be no doubt. This is proclaimed by the distinctive character of other portions of the opaque area, by its own relation to the retinal vessels, and by the striated appearance of its margins. The patient—a girl of moderately dark complexion—had no appreciable structural peculiarity in any other part of the body, and the other (right) eye, except for a slight degree of hypermetropic astigmatism, was in no respect noteworthy. The affected (left) eye, save for the opaque nerve fibres as above described, appeared normal, but with it the patient had no perception of light, and the pupil light-response was very defective. The eye was not divergent and there was no nystagmus.

Examples of such an extensive development of opaque nerve fibres as is seen in the case here illustrated (fig. 32), would appear to be extremely rare. Some years ago Mr. Hartridge[1] recorded an instance in which the disc was completely surrounded by opaque nerve fibres, with, in addition, a relatively narrow opaque band passing outwards and somewhat downwards for a considerable distance across the fundus. In Jaeger's Atlas, also, there is a somewhat similar illustration. The instance now reported is more extreme than either of these, both as regards the depth of the investment of the disc and the extent of the invasion of the macular and neighbouring parts of the fundus. It has also this further interest, that the patient's inability to see with the affected eye seems to be entirely due to the extraordinary development of opaque nerve fibres.

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[1] *Transactions of the Ophthalmological Society*, 1885, Vol. v.

In the second case (fig. 32), the peculiarity is the existence of two groups of opaque nerve fibres, not attached, as is usual, to the margin of the disc, but separated from this by a clear interval of normal fundus. Such an arrangement, in conjunction with patches having the more common situation, is represented in some of the ophthalmoscopic atlases. But in the present case the only existing patches are those placed in the fundus at some distance from the disc, the margin of the disc itself being in every respect normal.

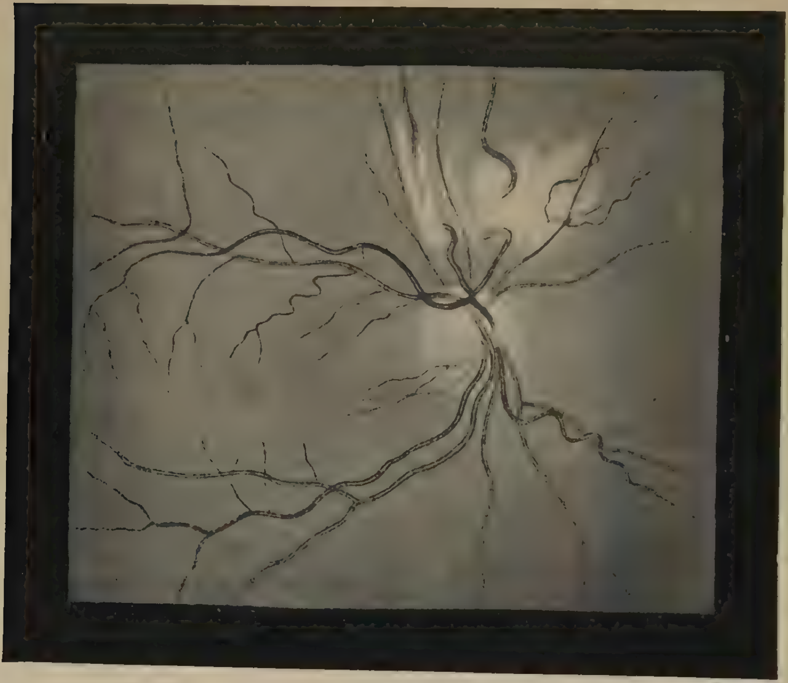


FIG. 32.—Patches of Opaque Nerve Fibres separated from the Optic Disc.

The patient was a youth of very fair complexion and reddish hair; his vision was normal, and neither in the eyes nor in any other part of his body was there any other structural abnormality. A case showing a single patch of opaque nerve fibres situated some distance to the inner side of the disc is figured by Mr. Work Dodd in the *Transactions of the Ophthalmological Society* (Vol. xx), and similar cases have been recorded by Mr. Adams Frost, Mr. E. Nettleship, Mr. Treacher Collins, and Mr. Sydney Stephenson.

In an annotation in the January (1904) number of *The Ophthalmoscope* the exceptional character of the facts represented in fig. 32 is emphasized. Casper, as there quoted, found a similar distribution of opaque nerve fibres in only four instances among 14,000 patients.

CICATRICIAL CONSTRICTION OF THE ABDOMINAL WALL AND OF THE LEFT THIGH ATTRIBUTED TO STRANGULATION BY THE UMBILICAL CORD.

THE unusual conditions existing in this case are clearly displayed in the accompanying illustrations (p. 259). As is shown in the figures, there is a narrow cicatricial band forming a complete girdle of constriction in the abdominal wall. This is present, on the posterior aspect of the body, at the level of the third lumbar spine; on each side it passes forwards and slightly downwards just above the level of the iliac crest; and the circle is concluded in front at a point 1¼ in. below the umbilicus. The cicatrix appears to involve only the superficial part of the skin, but it grips the abdominal wall to such an extent that it produces a groove, which, in the anterior part, is depressed from a quarter to half an inch below the surface. Behind, there is some appearance of a double groove, but this cannot be traced either in the lateral or anterior parts of the circle.

On the lower part of the left thigh, too, is a depression or furrow, which is placed obliquely immediately above the knee. The skin here shows no distinct cicatricial change, and the constriction is much less acute than that in the abdominal wall.

These conditions have, without doubt, been present from birth. Unfortunately it has not been possible to trace the practitioner who was in attendance when the child was born—she is now ten years of age—and therefore expert testimony in reference to the then existing facts is not available. But according to the mother's statement, delivery took place at the seventh month and the grooves on the abdominal wall and thigh were occupied by the umbilical cord. The same witness credits the doctor with the remark that "had the pregnancy gone to full time the child would have been cut into three pieces."

The chief feature of interest in the case is the question of the agency by which the grooves on the abdominal wall and thigh were produced. It may be assumed that at the time of birth these depressions were certainly occupied by the umbilical cord, because, apart from the mother's statement as quoted above, other cases have been recorded in which a loop of the cord has been found in somewhat similar constrictions, at least in the case of the limbs. Thus, it may be fairly argued that there is a *prima facie* case in favour of the umbilical cord as the constricting agent. Yet the evidence is not quite conclusive. For, it must be allowed, that given, during intra-uterine life, the formation of grooves upon the surface from any cause whatsoever, the mechanical conditions are such as would lead more or less readily to the reception and retention of the umbilical cord within such depressions. It is therefore possible that occupancy by the cord may be not the cause but merely an "accident" of these constrictions. Hence the question arises whether there is any other feasible constricting agent.

First of all in this respect comes the possibility of bands of tissue taking origin in connection with the formation of the amnion. Such bands are often regarded as the agents which lead to intra-uterine amputations of the digits or limbs, and some instances have been recorded in which they have been found constricting these parts. All that can be said in the present case is that there is no evidence of the existence or action of such bands. In the second place, there is the suggestion that constrictions and amputations of the limbs occurring during intra-uterine life, are due, not to some external agent, but to a trophic or sclerodermic lesion, which, while primarily affecting the skin, cuts its way, as the body increases in size, more or less deeply, and in

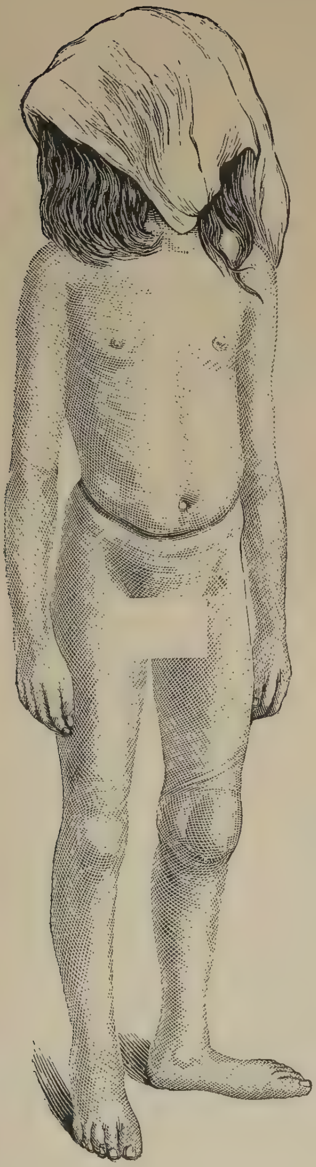


FIG. 33.

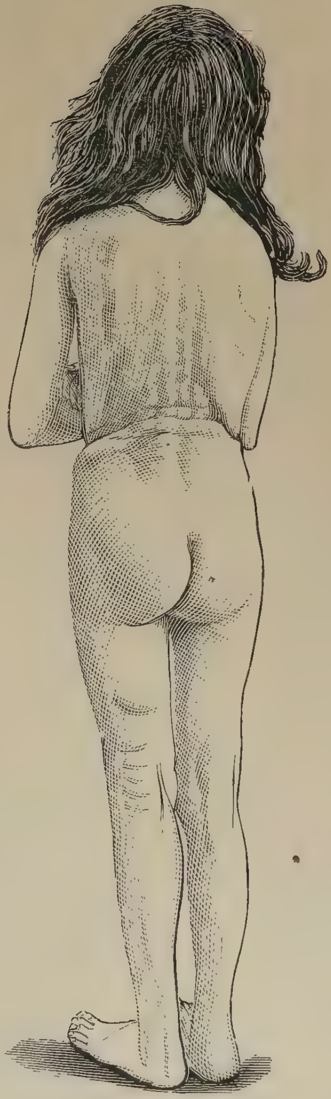


FIG. 34.

the most extreme instances causes strangulation, or even separation of the distal part.

Dr. J. W. Ballantyne, who discusses this question in his work on " Antenatal Pathology," considers that no " satisfactory explanation of the production of the so-called spontaneous amputations has yet been advanced." In view of so authoritative a statement some reserve in the expression of an opinion is necessary, but it may be permissible to suggest that in the present instance there are facts which support the view that here, at all events, it is the umbilical cord which is responsible for the abnormal conditions. Such facts are found more particularly in connection with the groove on the left thigh. This, it will be observed, has, not a circular, but a spiral direction and form. Now while it may readily be conceived that a trophic or sclerodermic lesion may produce a circular constriction of a limb, it is less easy to imagine an oblique or spiral groove, such as is here represented (fig. 33), to be the result of such a lesion. Further, the skin forming the floor of this groove, though perhaps somewhat atrophic, is apparently normal in every other respect; the appearances are just what might be expected from prolonged compression by a broad, smooth ligature. Add to these considerations the fact (as it may be assumed to be) that the cord was found in the groove at the time of the child's birth, and the evidence supporting the proposition that the cord is responsible for the existence of the groove, is, it must be admitted, very strong, if not actually conclusive. But if this view holds good in reference to the thigh, it must apply with almost equal force to the constriction of the abdominal wall. Only suppose a greater degree of tension, and both the more marked depression and the definite cicatricial change are readily explained. This hypothesis also accommodates itself to the association of a double groove on the posterior wall of the abdomen with the existence of a single furrow in the rest of the circle. For, in the former position, the firm structure of the wall opposes the formation of a deep depression, and thus a limited play of the cord from one level to another becomes possible; whereas the yielding nature of the lateral and anterior abdominal walls favours the ready production of a deep and narrow furrow, within which the cord would be retained and on the floor of which, therefore, its compressing action would be continuous.

The conclusion that the umbilical cord is responsible for the grooves or constrictions present in this case, bears upon the question of the causation of intra-uterine amputations. That the agency producing these is identical with that producing constrictions of the limbs is fairly certain. For, in addition to the common-sense presumption that amputation is merely constriction carried to an extreme degree, there is the experience that both conditions may be present in one and the same case. An example of this association was recently shown at the Polyclinic, the case being that of an infant born *minus* several fingers and with a deep constricting groove around one leg. But if intra-uterine amputations are, or may be, due to the same cause as that which produces constrictions of the limbs, and if, as is rendered in the highest degree probable from the present case, such constrictions may be caused by the umbilical cord acting as a constricting agent, it follows that intra-uterine amputations may also be accomplished by the cord or other band external to the fœtus. Hence the present case may be advanced as a contribution to the discussion regarding the mechanism of intra-uterine amputations. And its influence goes to show that such amputations are caused by an external band constricting in an extreme fashion the affected part. The argument, in brief, therefore, is, that intra-uterine amputations and constrictions of the limbs are produced by one and the same agency; that, as is here shown, such constrictions may be caused by an external constricting band—in this instance the umbilical cord; and that, consequently, such bands must be held responsible for the occurrence of intra-uterine amputations.

ON SUDDEN GREYNESS OF THE HAIR.

THERE is a widespread popular conviction that the entire hair of the scalp sometimes becomes grey in the course of a few hours, the change being usually attributed to the influence of acute mental distress or fear. Thus Byron makes the Prisoner of Chillon say :—

> "My hair is grey, but not with years,
> Nor grew it white
> In a single night,
> As men's have grown from sudden fears."

It is easy to trace a succession of events, which, by an almost natural exaggeration, may have given rise to this creed. Prolonged and extreme anxiety, without doubt, frequently tends prematurely to age its victims, and among its results may be a greater or less degree of blanching of the hair. A relatively rapid transformation in these circumstances may easily, under the influence of the dramatic sense, serve as a basis for the statement that the change took place "in a single night." It is fair to assume that this explanation is applicable to many reputed instances of sudden greyness of the hair, and even the frequently quoted cases of certain historical personages are not secure beyond reasonable doubt. Is it therefore probable that the generally accepted statement regarding sudden greyness of the hair must be dismissed as a popular exaggeration? While in reference to many individual instances this question may be answered in the affirmative, it is necessary to remark that there is in the common doctrine nothing which involves the element of physiological impossibility. On the contrary, there are certain experiences which go to show that it may quite well be true.

In the first place it has to be remembered that greyness of the hair is not simply a mere loss of pigment. The essential change appears to be some alteration in the nutrition of the hair, which leads, among other consequences, to the formation of minute bubbles of air in the substance of the shaft. With this, there is, no doubt, in the greyness which accompanies old age, a gradual atrophy of the pigment cells. But even here it would seem that the new fact which gives to the hair its characteristic grey appearance is the arrival of air globules in its tissue.

However this event is to be explained, it is certain, both that it may be of rapid development, and that it may be determined by nervous influences. There are on record many instances of the sudden appearance of patches of grey hair following mental shock or distress. Such patches may be seen on the scalp, eyebrows, or other parts. When once produced they often remain unchanged for years, or even throughout the remainder of life. It is, however, possible for such hairs to regain their coloration, and grey hairs which have appeared in the scalp during attacks of neuralgia, have, as a matter of fact, been observed, on subsidence of the pain, to return to their former appearance. When it is once realized that the immediate factor in the production of greyness is the development of small bells of air in the hair substance, and that this change may be a rapid, perhaps an instantaneous one, there is no difficulty in allowing the possibility or even probability of a complete, and perhaps, an equally rapid restoration. The way is thus open for an explanation both of reputed cases of complete greyness occurring in the course of a few hours, and also of instances of varying degrees of greyness in the same individual during comparatively narrow intervals of time.

But it is possible to go still further. May it not be the case that, as regards individual hairs, rapid or sudden greyness is the usual event? Certainly it is no infrequent experience to discover to-day a completely grey hair where none was noticed yesterday. And similarly, when in any particular area the degree of greyness varies from time to time, the explanation is probably to be found, not in the gradual and alternate fading and deepening of the colour of all the hairs, but in the appearance or disappearance, as the case may be, of the character of fully developed greyness in a greater or less number of individual hairs, the others remaining without change. No doubt in one and the same hair parts which are quite grey may be found associated with other parts which retain their usual coloured appearance. But in such circumstances the coloured and the grey portions are, at least often, separated, not by an intermediate faded area, but by a sharp line of demarcation; what has happened evidently is, that while one part has become completely grey, the rest of the hair has remained without change.

These facts seem to justify the suggestion that, so far as each individual hair is concerned, the development of greyness is a sudden event. This view is quite consistent with the experience that the hair of the scalp as a whole usually turns grey very gradually. The two positions are at once reconciled by the supposition, that, in ordinary circumstances, the sudden change affects different individual hairs, not on the same, but on successive dates. If this is a correct statement of the usual course of events, the alleged instances of sudden greyness of the whole scalp offer no very serious departure from the normal. Their only peculiarity is to be found in the number of hairs affected on one and the same date. So long as greyness is taken to mean mere loss of pigment, its sudden development over the whole scalp, or indeed in the individual hair, seems incredible. But in view of the known explanation of the immediate cause of greyness, of the admitted fact that grey patches may develop suddenly under nervous influences, and of the suggestion that sudden greyness is the physiological time-movement for the individual hair, the evidence, so far from excluding the possibility of sudden greyness of the whole scalp, renders such an event not improbable. Of course, individual instances must be established by competent testimony, and this is usually lacking. But there is no *a priori* reason for dismissing such cases as mere examples of over-statement arising from a popular love of the marvellous.

A single remark on the general physiological significance of the above facts may here be added. The change in the hair structure which leads to greyness is produced, without doubt, by some alteration in the nervous control of the hair tissue. The suddenness of the change, which in certain instances is not in dispute, must be taken to exclude the possibility that it is caused through the agency of the blood-vessels, and by a consequent modification of the nutritive supply. It must therefore be due to an influence acting directly through the nervous system. The circumstances thus offer a very impressive demonstration of the immediate and universal sway exercised by this system over the nutritive processes of the body. Considering that the tissues are constituent parts of one organic whole, that their individual growth and reproduction are dependent largely on their nutrition, and that these activities are conducted, not selfishly, but in subordination to a common end, such nutritive control is an obvious necessity of the position. A failure of the control and stimulus of the nervous system over tissue nutrition is made manifest in various diseased processes, and more particularly in those grouped under the term trophic lesions. Greyness of the hair can hardly be called a disease, but its occurrence as a direct consequence of nerve disturbance has special interest as showing that even tissues most remote from the central source of nerve power, and free histologically from nerve elements, are not beyond the authority which binds the various parts of the body into an organized and harmonic unity.

NOTE.

A very conclusive demonstration of the sudden development of greyness of the hair over a large area, and of the dependence of this condition upon a central nervous lesion, has been recorded by Sir William Gowers (*Lancet*, November 2, 1901, p. 1175). The patient was the subject of traumatic meningeal hæmorrhage over the left hemisphere, and during the three days on which he lived after the injury, the right half of his brown moustache and beard became blanched, so as to be almost perfectly white. The hair of the scalp was not affected. It was observed after death that the blanched half of the beard was separated from the unaltered brown half by a narrow, vertical, mesial zone, in which the hair was almost black. The suggestion is advanced that "the disordered innervation so changed the secretion at the roots of the hairs as to produce a material capable of ascending the hairs and discharging their pigment."

A parallel case, the result of mental shock, is reported by Dr. Augustus W. Addinsell (*British Medical Journal*, May 17, 1902, p. 1205). A woman, aged 22, was an involuntary witness of a terrible tragedy, and next morning found that the hair on the right half of the pubes had become perfectly white. Further examination revealed disappearance of the pigment from the right labium majus, and from the inner side of the right thigh.

An interesting communication by Dr. Robert Jones, and dealing with the subject of grey hair and emotional states, will be found in the *Lancet*, March 1, 1902, p. 583.

THE AUTHORITY OF THE PHARMACOPŒIA.

THE authority of the Pharmacopœia is both frequently quoted and universally allowed, but the nature and extent of this authority are not always so clearly recognized as is to be desired. Within its legitimate domain the Pharmacopœia is absolute, and none may dispute its commands. This domain, however, has well-defined limits, and it is not in the interest either of the medical profession, or—what is more important—of the general public, that these limits should be extended. There is exhibited at times a tendency to permit this, or even to invite it, and this essay is an endeavour to promote a general body of opinion against any such policy.

The one definite purpose for which the Pharmacopœia exists, is to secure within the British Isles an exact and uniform interpretation of the various terms used in physicians' prescriptions. The application of this purpose may possibly be usefully extended by modifying certain parts of the Pharmacopœia to meet the peculiar wants of other portions of the Empire. But the prime and dominating object of the book is not altered by such modification. The main design remains, namely, to provide an authoritative list of definitions of the names of medicines as these are commonly written by physicians. And the whole plan and arrangement of the Pharmacopœia are directed to the attainment of this end. The various medicines are first named. Each is then exactly defined in reference to its botanical or chemical source. A careful description of each is next given, so that the pharmacist may be in a position to detect any attempted substitution or adulteration, and may be able to satisfy himself that the substances with which he is dealing are really of the nature and quality demanded by the Pharmacopœia. When appropriate chemical tests are possible these are introduced, in order to afford an opportunity for the exact determination of both qualitative and quantitative values. In similar fashion the process of manufacture and the proportionate composition of the various medicinal preparations—compound or simple—likewise receive official definition. Hence, and as a result of these details, every dispensing chemist is provided with the machinery necessary to enable him to make sure that he is supplying the medicines ordered in the terms of any prescription as these are defined in the pages of the Pharmacopœia.

The justification of giving to the Pharmacopœia the sanction and authority of the law of the land, is, that no less an authority is competent to enforce a universal rule defining the sense in which the names of medicines are to be understood. Without the compelling force of legal authority, there could therefore be no secure guarantee that every dispenser would interpret these names uniformly, and in the sense intended by the physician. Neither the writer of the prescription, nor his patient, could be certain that the dispenser would understand its terms in the sense in which these were employed by the physician. The confusion, and indeed danger, involved in such a state of matters would obviously be opposed to the public welfare and safety, and it is therefore in the public interest that the Pharmacopœia exists, and that its decrees are promulgated with the weight and authority of the Legislature and are enforced by the sanction of legal penalties. But when the law has once secured this end its functions are discharged. It has provided for the public a means of protection which cannot be provided from any other source. To this extent legal force is necessary, because no other force is available. But any extension of the action and responsibility of the public authorities in this direction, is, it is here contended, neither necessary nor advisable.

That these principles are recognized by the Legislature is manifest from the terms of the Acts of Parliament under which the Pharmacopœia has come into existence. They are not, however, always recognized by the critics. On the contrary, responsible suggestions have more than once been made to effect a radical change in the character of the Pharmacopœia, and to determine its construction on principles entirely out of harmony with those which form its essential justification. For example, it has been proposed to limit admission to its pages to medicines approved by a committee of pharmacologists and physicians, and thus to make it a list of substances which, in the judgment of such a committee, possess accredited remedial value. It is not necessary to question the value of a volume constructed on these lines. But such a volume, whatever its merits, would not fulfil the purpose of a national Pharmacopœia. Nor is it in harmony with public policy that any such volume should have the support and dignity of legal authority. The object of the Pharmacopœia is not to say what substances *should* be used as medicines, but to define those that *are* used, and in this way to protect the public against the evils necessarily attached to any substitution, adulteration, or uncertain strengths of medicines. If a committee of pharmacologists or other experts desire to communicate their experience and views to the profession, there are numerous and ready opportunities for them so to do. They may prophesy as they will, and their opinions will no doubt receive due attention. But there is no reason at all why their teaching should be authorized and dignified by legal warrant and title. Indeed, any such proceeding is not merely superfluous; it is also most inexpedient, as it would tend to restrict the free play of private judgment in the selection and use of remedies and to create a narrow and artificial standard capable of being quoted as the measure of therapeutic orthodoxy. It is certainly not the business of the Legislature to determine and proclaim what is and what is not of definite therapeutic value, and any official attempt to do so would rather oppose the public interest than promote it. But it is the business of the law to secure for His Majesty's lieges a reasonable guarantee that the names of medicines shall each be construed in one and the same sense in every part of the country, and to punish those who carelessly or wilfully misconstrue them.

The General Medical Council has therefore been instructed and authorized to issue a volume of definitions and descriptions of those medicines which, in the judgment of the Council, require such definition and description. Now, obviously, these requirements apply to all medicines which are more or less generally used by physicians. Hence the Council, in framing the Pharmacopœia, has not to decide the therapeutic value of remedies—a function for which, in its corporate capacity at least, the Council is in no sense fitted—but rather to say what substances command such an amount of professional support as to make it wise in the public interest to provide them with statutory definitions. In the discharge of this function it is usage rather than usefulness which must guide the Council. No doubt it is desirable that the profession shall employ only those medicines which possess distinct therapeutic value. But the education necessary to promote this end must be left to the voices of the experts and to the discipline of experience. It is not within the province of the Legislature, or of the General Medical Council, or of the British Pharmacopœia. The special province of these in relation to the use of medicines is to obtain for the public a uniform interpretation of the terms which physicians employ in the construction of their prescriptions. In this way alone can the public safety be secured. The experts may properly employ themselves with the education of the profession, and may thus, indirectly, and in time, determine the constitution of the Pharmacopœia. The immediate duty of this book, however, is not to proclaim the current notions of pharmacologists, but to protect the public against the risks of misconstruction of the terms employed by the physician. Any action, therefore, which would limit the practice of official definition to a more or less narrow group of substances approved by laboratory or other arbitrary tests, is opposed to the public interest. The interest of the public is that the individual physician shall have full liberty to prescribe whatever in his judgment seems likely to lessen the patient's suffering and to promote his health; and that substances so prescribed shall be dispensed of the nature and quality intended by the physician. These ends are met by leaving the selection of remedies to the physician, while the law, by providing an imperative standard of reference, directs the dispenser how to interpret the terms which the physician employs. This standard of reference is the British Pharmacopœia.

The objections above stated apply with equal force to the suggestion that each of the substances named in the Pharmacopœia should have attached to it a list of the clinical conditions in which it may advantageously be employed. This, of course, would be to make the Pharmacopœia a volume of official therapeutics, and to constitute the General Medical Council an authoritative voice on the treatment of disease. It is not difficult to imagine how a book so constituted would restrict the liberty and invade the judgment of the practitioner, and how readily it would come to be quoted by the public, either in favour of, or in opposition to, a proposed scheme of treatment. Those who countenance such a proposal have evidently altogether failed to grasp the essential principle which demands and justifies the existence of a national Pharmacopœia. The same remark may be made regarding suggestions that the Pharmacopœia should contain useful hints to practitioners regarding the combination of remedies, the use of flavouring agents, and so forth. All these can be provided, and are provided, by private enterprise, far better than by the slow, cumbrous, and dogmatic processes of legal authority. But private enterprise cannot give an authoritative and compulsory list of definitions of the terms used in prescriptions, and cannot, therefore, secure the public against the dangers which the absence of such definitions would permit. Hence the Pharmacopœia must exist, and its directions must possess legal force and claim. These directions, however, apply within a comparatively narrow field, and the constituency to which they speak is rather that of the dispensers than the prescribers of medicines. The Pharmacopœia is neither intended to be a list of medicines guaranteed by professional authority to possess remedial virtue, nor a guide to the construction of elegant and successful prescriptions. No doubt there is a certain presumption in favour of pharmacopœial medicines, at least to the extent that as they are recognized in the official volume they must be more or less generally used, and if used, probably therefore useful. Such a consideration ought to have due weight with prescribers, and more especially with those of junior experience. The chief interest of the physician in the Pharmacopœia, however, is to see that the remedies he is in the habit of prescribing are adequately defined in its pages, in order that he may be able to rely upon the accurate and uniform interpretation of his prescriptions by the dispenser in whatever part of the country the latter may practise. The Pharmacopœia has no control over the selection of remedies for the individual patient; the decision in this respect is to be taken entirely on the judgment and responsibility of the physician.

What is true of the choice of remedies in the treatment of an individual patient, is true also of the determination of the doses in which remedies are ordered. Here, again, the judgment and responsibility are entirely matters for the decision of the physician. It is no doubt the case that for each of the official remedies used for internal administration, a minimum and maximum dose is quoted in the Pharmacopœia. But this is hardly for the guidance of the physician, and is certainly not for his compulsion. The statement, for example, that the dose of quinine sulphate is 1 to 10 grains, is, as a therapeutic lesson, a mere absurdity. And in practice, the so-called official doses are set at naught in a hundred instances every day. The one advantage which the introduction of doses into the official pages possesses, is, that it forms an additional check against the risk of accidental poisoning. It does this by making the dispenser officially acquainted with what may be called the average doses of the various remedies. Thus he is placed in a position to observe any slip or apparent slip, which, in this respect, may be made in the prescription, and to approach the physician to have this either rectified or approved, as the case may be. It is, indeed, only fair to the dispenser that when a dose in excess of the pharmacopœial maximum is intentionally prescribed, such a dose should be specially initialed or otherwise confirmed.

There is something to be said on the other side. The statement in the Pharmacopœia of average doses of the official remedies has indeed the advantage just claimed for it. But it carries certain penalties. It condemns the medical student to the laborious task of committing to memory and producing for examination purposes long lists of figures which do little either to illuminate his mind or increase his efficiency. And what is worse, the habit thus induced is very apt prejudicially to stereotype his practice in after years. The doses he has learned as a student become the unbending rule of his life, whereas successful therapeutics depends largely on the adjustment of doses to the requirements of the individual patient. And the wants of individual patients in this respect vary within very wide limits. The claims of public safety are necessarily imperative, and as the official doses help to promote this end, probably no one will advocate their suppression. But it is certainly wise to take note of the possible risk of their tyranny and to suggest that their true position and significance should be appreciated.

It may be said that all this seriously reduces the dignity and influence of the Pharmacopœia. Even if this be so, it is of little consequence so long as the real meaning and value of the volume are apprehended. In its legitimate sphere it is paramount. But to propose to develop it as the fine flower of expert pharmacological knowledge, or to convert it into a manual of directions for the prescriber, is to miss its essential purpose and to deprive it of its reasonable claim and defence. The one point where it touches the physician is that it compels him, in employing any one of its terms, to use this in the sense defined in its pages. In all other respects he must both exercise the freedom of his own judgment and accept the responsibility of his own decisions. The Pharmacopœia neither directs the one nor lightens the other. As an instrument in the service of the public it can boast a useful and valued function. But, so far as the medical profession is concerned, it is neither to be hailed as a dictator nor acclaimed as a fetich.

GALENICAL PREPARATIONS.

The term galenical, as applied to medicinal preparations, is not, perhaps, susceptible of very exact definition, though in commercial pharmacy it enjoys a well-understood practical meaning. Speaking generally, it may be said that galenical preparations are those obtained by such mechanical processes as mixing, dissolving, filtering, sifting, etc., as contrasted with preparations produced by pharmaceutical operations which involve recognized chemical reactions. Thus, extracts, infusions, tinctures, ointments, etc., are galenicals, while the salts of the metals are examples of chemical preparations. The chief interest of the term, however, is to be found in its historical associations. It refers, of course, to the practice and authority of Galen. And the term chemical is its historical as well as its commercial antithesis. Set in opposition, the two words revive the controversies of the fifteenth and sixteenth centuries, when the traditional practice of using remedies of vegetable and animal origin was questioned by the followers of Basil Valentine and Paracelsus, and when these, on the contrary, advocated the use of medicines derived from the mineral kingdom, or chemicals. Hence arose the rival schools of *Galenists* and *Chemists,* with their respective medicinal equipments of galenical and chemical remedies. The controversy, as is well known, was both noisy and long drawn out, and its din extended outside the limits of professional circles. A line in "All's Well that Ends Well" suggests, indeed, that the dispute was familiar to the general public. The reference is in Act ii, Scene 3, and occurs during a discussion concerning the alleged fatal illness of the King of France, whom Lafeu describes as "relinquished of the artists," to which comes a reply from Parolles: "So I say; both of Galen and Paracelsus."

The phrase "chemist and druggist," still in common use, and having in addition a recognized legal significance, is another modern survival of the controversy which once raged so fiercely between the respective advocates of mineral and organic remedies. Chemicals were originally, and are even yet in the popular sense, substances of mineral origin. And in its etymological sense a drug means a herb dried for use as a medicine. It is not uncommon, even to-day, to read in an advertisement of "pure drugs and chemicals," and the presentation of these names in such a fashion, marks, even yet, the nominal separation of remedial agents into two groups according as they own respectively a vegetable or a mineral origin.

The progress of chemistry, and the isolation from organic substances of alkaloids and other bodies having a definite chemical structure and capable of combination with acids, etc., to form salts, have broken down the line of distinction which formerly separated the two classes of remedies. Indeed, it has become common in recent years to apply the word "drug" to all materials used as medicinal agents. And obviously, quinine sulphate, for example, is as much a "chemical" substance as, say, magnesium sulphate. The advance of knowledge, and the toleration resulting from experience, have removed many of the artificial antagonisms of former disputes. But the echoes of certain of these can still be heard in the contrasted terms galenical and chemical, and also in the phrases drugs and chemicals, and chemist and druggist.

ON DRUG IDIOSYNCRASIES IN RELATION TO THE DEFINITION OF OFFICIAL DOSES IN THE PHARMACOPŒIA.

THE personal factor provided by the individual patient, forms, in medical practice, an element whose range and command can hardly be overstated. It weighs largely in the choice of a physician, and sometimes, with equal unreason, determines his dismissal; it modifies the expression of the processes of disease, and thus complicates the art of diagnosis; and last, but by no means least, it influences the actions of medicines, and therefore demands an attempted estimate in the construction of a scheme of treatment. Hence it must ever remain true that the prescription of remedies in the treatment of disease can never become a mere mechanical art. For, even when remedies have been accurately accommodated to definite and exact standards, patients will continue to differ, and this difference will express itself, in a more or less conspicuous fashion, in the shape of a varying response to the influence of drugs.

The existence of personal idiosyncrasies in relation to drugs is a doctrine which receives at least a measure of recognition in the discipline of medical education. But there may be a question whether it might not advisably be presented with greater emphasis. For the most part it is illustrated by the announcement that occasional individuals are found who are excessively susceptible to, or extraordinarily tolerant of, some few of the more potent agents among the official materia medica. Drug idiosyncrasies, in short, are presented as extraordinary or exceptional occurrences which appear as "sports" amid the generally stable and constant phenomena of the therapeutic world. So far from being merely extreme expressions of a general law, these illustrations are introduced as examples of the negation of all law. They are apt to appear, like so many big gooseberries, rather to enliven the dull season of a course of lectures on materia medica than to enforce a practical therapeutic lesson. So considered, it must indeed be admitted that instances of unusual sensitiveness to such potent remedies as opium and arsenic have at least some value, for they teach the student and junior practitioner to be careful in the use of these remedies. But it may be submitted that this is only a limited application of the significance of such experiences.

The suggestion here offered is that these events are merely extreme and conspicuous examples of a general truth, namely, that each individual patient has a degree of reaction, special and personal to himself, in relation to each member of the materia medica. The majority of individuals may, it is true, closely approximate in this respect to a common standard, but between the extremes of excessive susceptibility and excessive tolerance, all gradations are possible, and successful therapeutics largely consists in the determination of the exact position of the individual patient in relation to the individual remedy. The probable truth of this statement may well be expected *a priori*. If one individual is found to react powerfully to a minute dose of atropine, for example, while another receives a relatively large dose without manifest change, it is surely probable that individuals exist who form a graded series between these two extremes. And equally it is probable, that what is true of one drug—for example, atropine—is true in a greater or less measure of all others. This proposal, further, has the support of experience, and hence it seems desirable to emphasize conspicuous drug idiosyncrasies not as extraordinary and exceptional occurrences, but as impressive illustrations of a general law, the existence of which applies over the whole field of drug administration. Such a presentation involves an exhortation to the prescriber to free himself from the yoke of a rigid and authoritative scale of doses, and to make himself ready, both in one direction and the other, to adjust his quantitative orders to the idiosyncrasies which exist, not in exceptional cases only, but in each individual patient.

The question now to be asked is: What influence is exerted upon the doctrine of drug idiosyncrasy as just defined, and upon the practice which is its natural and inevitable outcome, by the erection of an official standard of doses in the national Pharmacopœia? The first remark which arises on such a question is that the two schemes appear to be in absolute opposition to one another. In one, flexibility is the predominant note; in the other, rigidity. It is here submitted that as a matter of fact the pharmacopœial doses do exercise a prejudicial influence upon the habit of the practitioner in the quantitative adjustment of his remedies in dealing with the individual patient. These doses are for the most part committed to memory by the student in anticipation of the demands of the examiner. A similar consideration determines their appearance in the pages of the current manuals of materia medica and therapeutics. And the examiner, it may be hinted with all due deference, not infrequently adopts an attitude which justifies these procedures. Thus in the early and flexible phases of the practitioner's career a more or less arbitrary scheme of doses is stamped upon his memory, and these are the more impressive as they appear to enjoy an authoritative sanction. Is it surprising to find that these powerful influences, exercised on plastic and receptive minds, determine permanently the habits of a lifetime, and that the rigid rules which may be necessary for the inexperienced remain to limit the freedom of the man of full stature?

The argument therefore is, that the existence of an official scheme of doses, so far from favouring the cultivation of a free and responsible personal judgment in the selection of the quantities of remedial agents suitable for administration, tends to oppose such a habit, and to encourage mechanical and arbitrarily-restricted methods of prescribing. It suggests to the practitioner an appeal to authority rather than a study of the personal peculiarities of the patient, and it indicates a limitation of dose by precept instead of by physiological and therapeutic effects. The dangers of over-dosage attract many warnings, but who shall tell the disasters which have attended the timid and restricted prescriber? If, therefore, as will be admitted, the careful adaptation of remedies, both qualitative and quantitative, to the wants and idiosyncrasies of the individual patient is an essential part of good practice, and if, as will also be admitted, each individual patient has his own claims in this respect, it is obvious that conformity to a stereotyped scale of doses is out of harmony with the liberty and power of scientific prescribing. In other words, the existence of a scale of official doses encourages neglect of some of the conditions which favour the successful use of drugs in the treatment of disease.

Now the logic of all this would appear to be an appeal for the removal of the official doses from the pages of the Pharmacopœia. Were the desired end to be attained only by such a step, the present writer would not hesitate to adopt it. At the same time, it would have to be recognized that this policy would remove one of the arrangements by which an accidental error of dose may be prevented. The existing plan affords an added protection to the public by forming a platform on which the dispenser can approach the physician should the latter, by a slip of the pen, have ordered a dose manifestly excessive and dangerous; and, in view of this, the suppression of the official doses cannot be lightly advocated.

Further, it may be urged that the disadvantages which are attached to the existence of the official doses, and which have already been discussed, may be avoided by a method short of wholesale abolition. These stated doses may remain as a convenient opportunity for communication between the dispenser and the prescriber, but they should be plainly recognized as having little other significance. In particular, they should be deprived of any suggestion of official force. In the preface to the Pharmacopœia, it is true, the real position and significance of the so-called official doses is recognized and defined. But the limitations and qualifications there introduced are not vividly before the minds of medical students, nor are they always emphasized in the manuals of materia medica or in the discipline of examinations. On the contrary, the official doses in these directions too often appear exclusively to hold the field. The result, if the earlier argument of this paper is sound, cannot fail to be, —as is here affirmed—unfavourable to that condition of successful prescribing which demands adjustment of the dose administered to the personal idiosyncrasies of the individual patient.

Thus, while in the interests of the public safety it may be necessary to allow the Pharmacopœia to continue to print a maximum dose for each of the official remedies used for internal administration, these doses should be presented in their true significance and should be deprived of any orthodox halo which the accident of their position may appear to attach to them. Towards the same end it is advisable that strenuous resistance should be offered to any attempt to make the Pharmacopœia other than an authoritative interpretation and definition of the meaning of terms currently used in physicians' prescriptions. And, in particular, no countenance should be lent to the ambition to constitute the Pharmacopœia a collection of remedies boasting the possession of a pharmacological guarantee, or to include within its pages a series of therapeutic directions and suggestions. It is in the interests both of the public and of the profession that the individual practitioner shall accept personal responsibility alike for the remedies he uses and for the doses in which he prescribes them, and these ends would not be favoured by the appearance of a volume claiming public and authoritative therapeutic sanction. So long as the national Pharmacopœia is kept in its present comparatively humble position the existing practice of stated doses may be permitted, provided due note is taken of the dangers involved, and adequate measures are adopted to prevent what is a convenience for the prescriber and dispenser being converted into a restriction on practice. In short, a series of official doses must be recognized as a possible danger to free and responsible personal prescribing, unless at the same time its functions and purpose are strictly limited and defined, and unless, also, emphatic proclamation is made of the truth that scientific and fruitful indications for practice are to be found, not in the general terms of the doses stated in a Pharmacopœia, but in careful and thoughtful study of the personal idiosyncrasies of the individual patient.

CLINICAL LECTURES AND DEMONSTRATIONS

A CLINICAL LECTURE ON A CASE OF FUNCTIONAL HEMIPLEGIA.

GENTLEMEN,—The patient to whose case I wish to direct your attention this afternoon, is a man aged 31. He is, as indeed is at once manifest, the subject of left hemiplegia; and this condition he believes to be due to a blow on the head which he received some fifteen months ago. The principal questions for decision are (1) the nature and (2) the site of the pathological disturbance responsible for the paralysis. To answer these questions, we need, of course, the history of the case and a complete examination of the patient.

The history is one of great interest. According to the patient's own statement—and this is confirmed by the testimony of his friends—he was, previous to November 29, 1904, in the enjoyment of good, or even robust health. On that date he was a passenger on a tram-car, between which and a passing cart a collision occurred. The shaft of the cart broke one of the car windows near to where he was sitting, and his face on the left side was slightly cut by some of the broken glass. He believes, moreover, that the shaft struck the back of his head on the left side, and it is certain that immediately or shortly after the accident he became unconscious and was taken to an infirmary, where he was detained until the following morning. He was then allowed to go home, and though "very shaky," had no other ground of complaint. His condition remained without change during the following week, but at the end of this time he began to complain of giddiness, and of deafness in his left ear. For these symptoms he consulted Mr. Chichele Nourse, at the Central London Throat and Ear Hospital, who certified the case as one of "nerve deafness." Early in January, 1905, he had a fit when walking in the street, the attack being apparently attended by complete unconsciousness; a similar event has recurred on several subsequent occasions, and in one of these "fits" he is said to have passed urine involuntarily. About this time, too, he was noticed to drag his left foot when walking, and soon afterwards he lost power in his left arm. In consequence of these later developments Mr. Nourse referred him to the North-West London Hospital, where he came under my care. The personal history may be briefly completed by the statement that at no time has the patient had any serious illness, that he has never used alcohol to excess or suffered from any form of venereal disease, and that his present symptoms have been entirely unattended by any disturbance (except on the one occasion already mentioned) of the functions of the bladder or rectum.

This being the clinical history, we have next to consider the examination of the patient. On the negative side this shows entire absence of all evidence of visceral disease. The cardiac sounds are normal, there is no sign of arterial degeneration, the urine is free from albumin, and the ophthalmoscope shows on each side a normal fundus oculi. Here, then, at all events, there is nothing to suggest the diagnosis of any one or other of such well-recognized causes of hemiplegia as cerebral hæmorrhage, thrombosis, embolism, or tumour.

On the other hand, there exist a number of signs and symptoms which point confidently to the construction of a positive diagnosis. First, the hemiplegia, though much less pronounced than was the case three months ago, is still a conspicuous fact, and it is to be noticed that it involves the left face as well as the limbs, and that the disability of the leg is more pronounced than that of the arm—this latter condition being in contradiction to what is the common experience in cases of hemiplegia. The most significant facts, however, are discovered when the sensory functions are tested. It is then found that there is absolute anæsthesia over the left upper and lower limbs, and also over the left side of the face and trunk, the anæsthesia, at all levels, ceasing abruptly at the middle line. Whatever form of stimulus is applied, even a powerful faradic current or transfixation of a fold of skin by a large needle, the patient remains absolutely indifferent. That is, in addition to left hemiplegia there are also left hemianæsthesia and left hemianalgesia; and these latter conditions affect, not merely the superficial, but also the deeper tissues, so that the most energetic grip applied to the muscles produces neither recognition nor complaint. Further, the anæsthesia extends to the left half of the tongue and buccal mucous membrane, to the interior of the left nostril, and, in a lesser degree, to the left conjunctiva. And yet again, it is not only common sensation and the sense of pain which are wanting; but equally, the special senses also are decidedly defective. Thus, with the left eye the visual acuity is barely 6/12, while the right eye has normal vision; the left ear is almost absolutely deaf, due, as Mr. Nourse tells us, not to disease of the middle ear, but to some defect of the nervous apparatus; and the patient has no sense of smell through the left nostril, nor of taste with the left half of the tongue. Another symptom, likewise capable of being regarded as a functional defect of the afferent nervous system, is loss of the sense of position; the patient, when his eyes are closed, is quite unable to recognize or describe the positions in which his left limbs may be placed, nor can he imitate these by voluntary attitudes of his right limbs—that is, there is impairment of the muscular sense. In a word, the condition of left hemiplegia is associated with loss or defect of general and special sensation over the whole of the left half of the body.

Now it may at once be said, that when a hemiplegia is accompanied by a profound degree of hemianæsthesia, there is a reasonable suspicion that the case is one of functional and not of organic disease. Still more is this true when the sensory defect extends to all forms of sensation, including loss of the sense of pain. The suspicion is further strengthened when the anæsthesia involves the mucous membranes, and is confirmed when the special senses share in the defect. And yet again, when, in addition to all this, the sensory loss persists in spite of definite improvement in the muscular paralysis, the presumption in favour of functional disease is raised almost, if not quite, to the level of certainty. Every one of these forms of sensory defect can be demonstrated in our present patient, and therefore there is a probability, based upon experience, that the hemiplegia and other symptoms from which he suffers are the product of functional disturbance and not of organic disease.

On the other hand, in the great majority of cases of organic hemiplegia, sensory defects are either absent, or are slight and transient. It is true that extensive disease of the cerebral cortex may, in addition to hemiplegia, produce both hemianæsthesia and loss of the sense of posture; but the sensory defect in such circumstances rarely includes the sense of pain or of temperature, and so far from persisting for months, as in the present case, it tends to disappear, even although the motor paralysis remains. Hence the character and duration of the anæsthesia in our present patient, other facts apart, strongly oppose the suggestion of organic disease of the cerebral cortex.

But it may be said that all this does not apply to the internal capsule, in which, as they run side by side and close together, both the motor and sensory tracts may readily be affected by one and the same pathological disaster. And in reference to this it must be admitted, that though the great majority of capsular lesions involve the motor and not the sensory pathway (or this latter only in an indirect, slight, and transient fashion), it does occasionally happen, through an extension of the lesion into the hindermost part of the capsule, that definite hemianæsthesia is one of its consequences. Further, the hemianæsthesia in such circumstances may be, though it is not always, profound; it may involve, not only the skin, but also the deeper tissues; the special senses and the sense of posture may share in the defect; and in exceptional instances the hemianæsthesia has remained as a permanent condition and long after the motor paralysis has disappeared.

Now if these statements are accurate, the hemianæsthesia of our present patient is not unlike what is at least possible as the result of an organic lesion of the posterior part of the right internal capsule. It may, perhaps, therefore be suggested that this diagnosis would meet all the facts—the hemianæsthesia as well as the hemiplegia. In support of this position, too, it might be pointed out that the paralysis of the lower limb is more marked than that of the upper. For this is exactly what is to be expected if the lesion of the internal capsule is situated so far back as to involve the sensory pathway. In such circumstances the motor strands immediately in front of this pathway—that is, those concerned with the lower limb—will in all probability be more seriously damaged than those related to the upper limb and face, which lie further forward. In other words, in a hemiplegia accompanied by a hemianæsthesia, and both being results of a lesion of the internal capsule, it is to be expected that the lower limb will show more evidences of paralysis than the upper. Applying these doctrines to the present case, it might be argued, that admitting the degree, character, and persistence of the hemianæsthesia to be inconsistent with organic disease of the cerebral cortex, these features of the sensory defect can be explained by organic disease of the internal capsule; and further, that the more marked display of the paralysis in the lower limb rather than in the upper, decidedly favours this view.

Manifestly this position must be met, if the suggested diagnosis of functional disease is to be successfully maintained. And it can be met, almost if not entirely without qualification, by the results which appear from a study of the visual fields. These results are displayed in fig. 35, where the left field shows considerable contraction, while the right field is approximately normal. The proposition now affirmed, is, that the existence of visual fields having these characters is inconsistent with

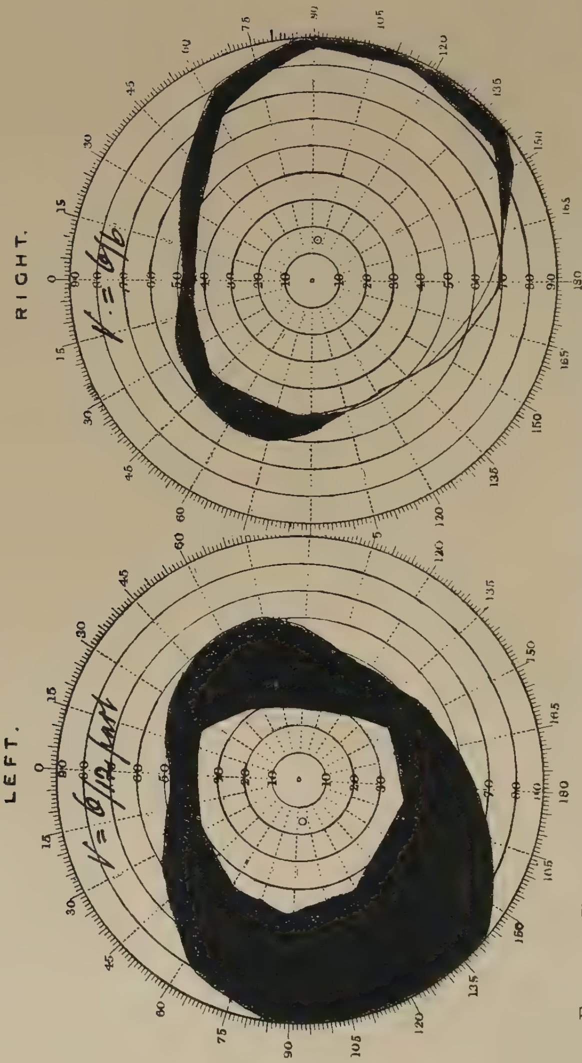


FIG. 35.—Charts showing contraction of left visual field (crossed amblyopia) in a case of left hemiplegia (functional), the right field being but slightly modified.

a lesion in the internal capsule. It is true that the normal pattern of the visual fields may be modified by such a lesion. But the modification would be of an entirely different character from that seen in the present case. For, in consequence of the partial decussation of the optic nerves in the optic commissure, the visual path behind the commissure (and therefore its transit through the internal capsule) is related, not to one eye, but to parts, corresponding parts as they are called, of the two eyes. Thus the right visual path in the right internal capsule represents the right half of each retina (fig. 36), and hence a lesion of the right capsule, if it involves the visual pathway, will abolish the function of the right half of each retina. But the *right* half of each retina is the part which receives rays of light from objects situated in the *left* half of each visual field. When, therefore, the function of the right half of each

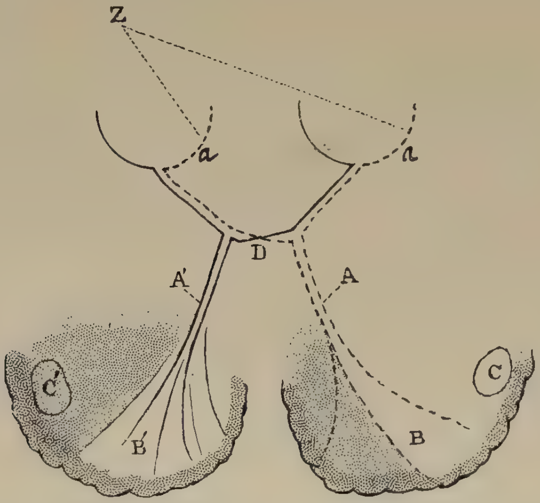


FIG. 36.—Diagram of visual paths. A = right visual path behind the optic commissure (D), receiving fibres from the right half of each retina (*a*, *a*) and terminating in right occipital lobe (B). C = higher visual centre on right side representing the whole of the opposite eye, and the presumed site of disturbance in crossed amblyopia (left). Z = object to *left* side reflecting rays of light which reach the *right* half of each retina.

retina is abolished, there will be blindness in the left half of each visual field (left homonymous hemianopsia). That is to say, the presence of a lesion involving the visual path in the right internal capsule will be announced by the existence of left homonymous hemianopsia. Such a representation is seen in fig. 37, the fields being those of a patient suffering from left hemiplegia the result of an organic lesion in the right brain. But in our present patient a very different state of matters exists. Here there is, it is true, an obvious defect in the left field (fig. 35), but it is a concentric, not a half-field or hemiopic defect, and it is not associated with any appreciable alteration of the right field. The condition of the visual fields, therefore, cannot be due to a lesion of the right internal capsule, as such a lesion, if it affected the visual path at all, would produce a lateral loss of each field, namely left homonymous hemianopsia. But if the visual defect cannot be due to a capsular lesion, the same proposition is in all probability true of the hemiplegia, for there is no reason to doubt that both the hemiplegia and the visual defect are expressions of one and the same pathological event. A lesion of the right internal capsule, in other words, does not satisfy the existing clinical picture.

So much, then, for the perimeter tracings in their negative significance. Their positive significance is almost equally definite. For, it is widely recognized, that whatever be the explanation, the visual conditions existing here, namely, defective acuity (amblyopia) and a contracted field, when associated with and on the same side as a hemiplegia (and therefore "opposite" or "crossed" in reference to the site of the presumed responsible cerebral disturbance), are in the highest degree suggestive of a functional explanation of the paralysis. Hence the defective acuity of our patient's left eye, and the contraction of his left field, are reasons for concluding, on the basis of clinical experience, that the cause of his paralysis is not organic but functional disease. Another feature of the visual field which points in the same direction is dislocation of the natural order of the colour fields,

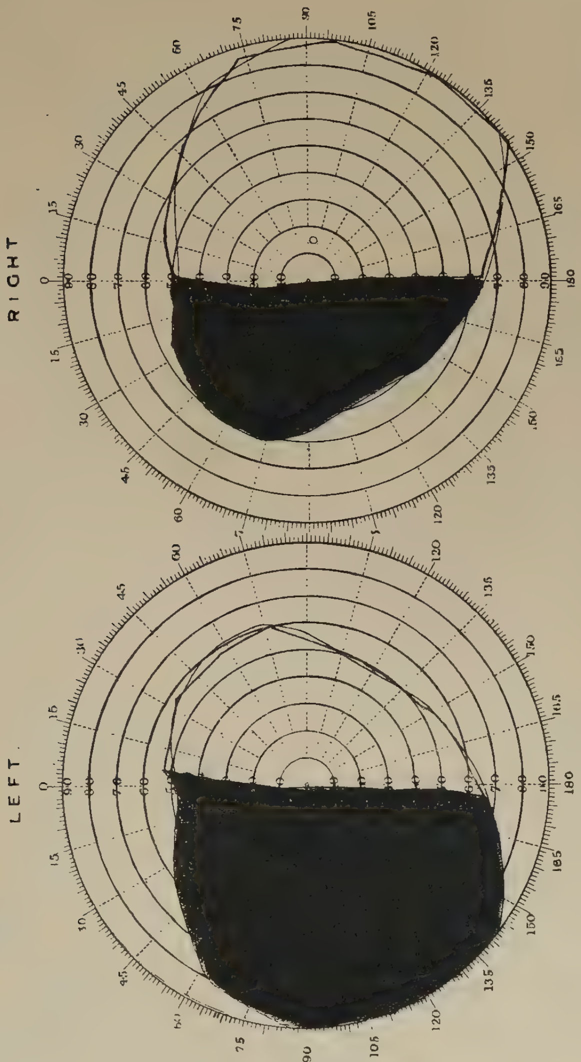


FIG. 37.—Blindness in left half of each field (left homonymous hemianopsia) in a case of left hemiplegia (organic).

as seen in fig. 38. Normally, from without inwards, the circles lie blue, red, green. But in the case illustrated in fig. 38 blue is the smallest of the circles and falls within the red. In our present patient such dislocation of the colour fields cannot be determined. Yet it is noteworthy, that though he says he has never been a good judge of colour, he recognizes the coloured cards of the perimeter with the right eye, but fails to identify any of them with the left. The study of the visual fields has displayed, therefore, a series of facts inconsistent with the diagnosis of organic disease of the internal capsule, but in substantial harmony with what clinical experience teaches to be a classical occurrence in functional hemiplegia.

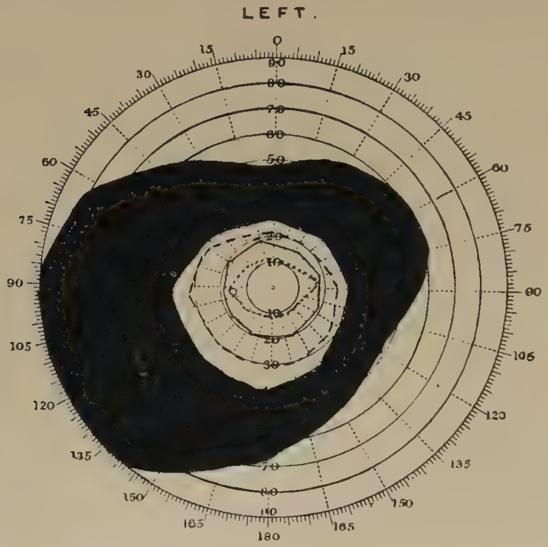


FIG. 38.—Contraction of left visual field with alteration of normal relations of colour fields, namely from without inwards; - - - - red, —— green, …… blue, in a case of functional hemiplegia. The paralysis was on the right side, and the visual acuity of the right eye was so reduced that no chart could be obtained. Patient, a female, was in Sir William Gairdner's ward in the Glasgow Western Infirmary, and I am indebted to Dr. Maitland Ramsay for a copy of the above chart.

It may, however, be asked whether the association of homonymous hemianopsia with organic disease on the one hand, and of crossed amblyopia with functional disturbance on the other, is an absolute and unvarying rule, or whether it admits of exceptions. Everyone, I think, will admit that if exceptions occur they are very rare, and hence the rule as just stated has very considerable diagnostic value. Nevertheless, reasoned anatomical and physiological conclusions in reference to the visual path do not absolutely forbid the possibility of such exceptions. Thus it is certain that the visual path—say the right for example—in its course behind the optic commissure, contains, as already explained, fibres from the right half of each retina, and that the right occipital lobe, in which it terminates, represents, therefore, the left half of each visual field (fig. 36). Hence, just as an organic lesion of the visual conducting fibres on the right side will produce left homonymous hemianopsia, so, likewise, will organic disease of the right occipital lobe. But will not, also, functional disease of the right occipital lobe produce left homonymous hemianopsia? For, if this form of disease may paralyse the action of one portion of the cerebral cortex, there is a reasonable presumption that any other portion of the cortex may be similarly affected. If functional paralysis of the motor or Rolandic area is possible, the same condition may also be possible in the occipital lobe. In the one case (presuming it is the right side of the brain which is disturbed), the result will be functional left hemiplegia; in the other, functional left homonymous hemianopsia. There is, in short, no *a priori* reason why a condition of homonymous hemianopsia should not be included among the consequences of functional disturbance affecting one of the cerebral hemispheres; and in rare and occasional cases this possibility appears to be realized.

Similarly, there is no compelling circumstance which absolutely forbids crossed amblyopia as a result of organic disease. Assuming that the amblyopia may be caused by a unilateral lesion or disturbance—and its association with hemiplegia entitles us to do this—it is obvious that the visual path must have relations to the cerebral hemisphere other than those presented by the occipital lobe. For the occipital lobe, as we have seen, represents, not the retina of the opposite eye, but half of the retina of each eye; and therefore, injury to, or defect in the function of, this half-vision centre, as it is named, must produce homonymous hemianopsia. If, however, a unilateral lesion is to produce, not hemianopsia, but crossed amblyopia, there must be somewhere or other in the cerebral cortex a centre which represents, not half the retina of the opposite eye but the whole of it. It may, indeed, be accepted that this higher visual centre (fig. 36, C) is to be found in an area immediately anterior to the occipital lobe—that is, in the hinder and lower part of the parietal lobe; and, presumably, it is some disturbance of this centre which is responsible for the crossed amblyopia found by experience to attend functional hemiplegia. Further, there seems reason to believe that this centre has also some measure of association with the eye of the same side, and thus functional disturbance of, say, the right brain, may lead, not only to contraction of the left visual field, but also, though usually to a less extent, of the right field. But if functional disease of this higher visual centre will produce crossed amblyopia, organic disease of the same region must be admitted to be capable of the same result; and exceptionally such a result has been recorded. Therefore it cannot be argued that a crossed amblyopia must, as a physiological necessity, mean functional disease and no other, any more than, as we have already seen, it can be maintained that a homonymous hemianopsia, for a like reason, necessarily signifies structural or organic change. But we can say that the records of clinical experience show, that in the vast majority of cases crossed amblyopia, in association with hemiplegia, has, as a matter of fact, meant functional disturbance, and homonymous hemianopsia with hemiplegia has, as a matter of fact, meant organic disease; and further, we are not altogether in the dark why this should be so. For it is reasonable to assume that the functions of the higher (and therefore less stable) visual centre will be more easily disturbed than those of the lower, half-vision centre in the occipital lobe; and hence, in a case of functional cortical disturbance, a crossed amblyopia is more probable than a homonymous hemianopsia. Given, therefore, an extensive functional defect of the cerebral cortex of one hemisphere; and crossed amblyopia, hemiplegia, and hemianæsthesia may well be associated as joint results of this defect.

Again, when organic disease affects the cerebral cortex so as to produce hemiplegia, the lesion is usually limited to the Rolandic area, and does not extend backwards so as to reach the hinder part of the parietal lobe; therefore crossed amblyopia in association with hemiplegia as an expression of organic disease of the cerebral cortex, is a highly improbable event. On the other hand, for reasons closely concerned with the vascular supply, organic lesions productive of hemiplegia are common occurrences in the internal capsule, where the visual path may possibly be involved, with homonymous hemianopsia as a necessary consequence. Therefore homonymous hemianopsia in association with hemiplegia suggests an organic explanation of the paralysis, as both events may readily be explained by a single lesion situated in the internal capsule. In the case of a capsular lesion, any question of a functional defect can hardly arise, for it is to be supposed that the suspension of function responsible for functional paralysis refers to nerve-centres, and not to structures (nerve-fibres) whose function is merely to conduct nervous impulses, not to originate them.

To sum up this part of the discussion, it may be said that crossed amblyopia is more likely to accompany functional than organic hemiplegia, because, first, an extensive functional disturbance of the cortex is far more likely than an extensive organic lesion of the cortex; and because, in the second place, the higher visual centre (fig. 36, C) is more easily disturbed than the lower, half-vision centre. On the contrary, as organic disease of the internal capsule is able to involve the visual path, homonymous hemianopsia is an argument in favour of an organic explanation of an existing hemiplegia, the only other possible explanation being either a functional or organic change so extensive as to involve the cerebral cortex from the Rolandic area in front to the occipital lobe behind, and disturbing the half-vision centre of the occipital lobe rather than the less stable higher visual centre of the parietal region. So extensive an organic lesion is improbable for obvious reasons, while with functional disease, both experience and physiological probability show that it is the higher rather than the lower visual centres which are disturbed.

The study of the visual fields in our present patient has therefore brought us to this position : it has excluded from the diagnosis organic disease of the internal capsule, and has raised the strongest possible presumption against organic disease of the cerebral cortex; and the latter conclusion is confirmed by the extreme and persistent anæsthesia. It is true that hemiplegia may result from lesions in situations other than the two just mentioned. But with such lesions there would be other localizing evidences—such, for example, as paralysis of one or more of the cranial nerves—and therefore these possibilities do not arise for consideration in the present case. Hence we seem driven to the conclusion that there is no proof of organic disease, as an explanation of the hemiplegia, in any part of the nervous system—that, in short, the paralysis is dependent, not on structural change, but on functional disturbance. And for such a diagnosis, the character, degree, and persistence of the anæsthesia, and the pattern of the visual fields, may be advanced as providing positive and substantial justification. Certain it is that these are the features which have distinguished many cases of hemiplegia whose clinical course and termination have been inconsistent with the existence of an organic lesion, whereas the ordinary hemiplegia of organic origin presents, in these respects, an entirely different clinical picture.

Further support for the diagnosis of functional disease may be found in the absence of all evidence of those diseased conditions of the heart, blood-vessels, and kidney which are known to lead to hemiplegia;

and the absence of a syphilitic history may be quoted for what it is worth in the same direction.

Of importance, too, are the circumstances in which the paralysis developed. The patient, immediately after receiving a fright, and perhaps also a blow, promptly becomes unconscious, but soon recovers; then, after some months, during which he was not "up to the mark," he gradually loses power in the limbs of his left side. This "incubation period," as it was called by Charcot, though not always occurring in cases of functional paralysis, is much in favour of such a diagnosis as compared with a suggestion of compression of the brain by a depressed fracture or by a blood-clot, which the circumstances of the accident in the present instance might well propose. Perhaps the fact of the paralysis being on the left side is worth adding to the diagnosis, for, in cases of functional hemiplegia, it is usually, though not invariably (fig. 38), the left side which is affected.

Granted, then, that the diagnosis of functional paralysis is justified, the case illustrates certain further points of distinct importance. First may be mentioned the occurrence of such a condition in a male patient, as a demonstration of the doctrine that functional disease is not confined to the female sex. Secondly, the history of the patient as an energetic worker and athlete, shows us that for a diagnosis of functional paralysis it is by no means necessary to have symptoms or manifestations popularly known as hysterical. In the third place, the case exhibits how suggestive of organic disease may be the accompaniments of a functional paralysis. An attack of unconsciousness, due, apparently, to a blow on the head, and followed at intervals by sudden seizures compelling the removal of the patient to hospital—then a progressive and widespread paralysis with complete loss of sensation in one half of the body, and with deafness and other defects of the special senses—these, indeed, form a combination of events well calculated to deceive the unwary. Yet, if the argument here advanced is sound, it is the very extremity of at least some of these events that affords a basis upon which the diagnosis of the hemiplegia as a functional condition can be safely rested.

There still remain three features of the case which closely concern this discussion, inasmuch as each one of them might be urged with greater or less insistence in opposition to the diagnosis of functional disease. First is the fact of facial paralysis. The paralysis here is quite beyond question; and it affects the upper as well as the lower facial muscles. Now it is no doubt true that the cranial nerves as a rule are not affected by functional paralysis; there are, however, exceptions, and the present case is one of them. It is said that even when facial paralysis appears to be present in functional hemiplegia it is not a true paresis; the muscles of the affected cheek are described as free from "the relaxation which is observed in organic hemiplegia." So far as I can judge, this description certainly does not apply to the present case. The appearances, indeed, are quite those of an ordinary paralysis affecting mainly the lower facial muscles, but also in some measure the upper muscles. Of course, the last-mentioned fact affords a contrast to what is usually seen in the facial paralysis which is part of an organic hemiplegia. But this statement cannot be advanced as an absolute one, for occasionally the paresis in such circumstances affects the entire side of the face. Altogether it must be said, that in the present case the only peculiarity which throws doubt on the diagnosis of organic disease, so far as the facial defect is concerned, is the fact that in spite of the hemiplegia, there is no deviation of the tongue to the paralysed side; if anything, indeed, the tongue when protruded is directed rather to the sound (right) side. Facial paralysis may, then, be noted as a feature of special interest in this case of functional hemiplegia.

The second exceptional fact is the presence of ankle-clonus. And here some of you may be disposed to part company with me, and to say that ankle-clonus cannot mean other than organic disease. Well, if that proposition is true, all I can say is that the diagnosis of functional paralysis in this case disappears, for a more definite and sustained ankle-clonus than is present here can hardly be imagined. And it is obtained, observe, not in the paralysed (left) but in the, presumably, sound (right) limb. A frequent statement is that a modified or false clonus may be obtained in cases of functional nervous disease, but that sustained and rhythmical clonus invariably means organic disease. That an obviously spurious or artificial clonus may be, and occasionally is, produced by hysterical patients there can be no doubt; this, however, is not at all likely to be confounded by anyone who has once seen it with the true variety. But, this pseudo-clonus apart, I am quite satisfied that a true rhythmical ankle-clonus, indistinguishable from the clonus of organic disease, can and does occur in some cases of functional nervous disease. The rule, no doubt, is the other way, and such a clonus should therefore certainly awake keen suspicion, but that its invariable interpretation is organic disease, is, I am convinced, from the experience of several cases, not true; and the present case confirms this statement. If this is so, it is of all the more interest to observe that the clonus here is obtained, not in the paralysed, but in the presumably healthy limb.

The last of the three events which I have described as possibly qualifying the diagnosis of the hemiplegia as a functional condition, is provided by the condition of the knee-jerk on the left (paralysed) side. The right knee-jerk is conspicuous, and probably in excess, but the left jerk at first appears to be absent. With some difficulty, however, it is obtained, but at best is of very limited extent. This, it must be admitted, is a somewhat serious observation, and that for two reasons. First, functional nervous disease is usually signalled by exaggeration of the knee-jerks. Secondly, serious defect or absence of the knee-jerk means, for the most part, organic disease. Hence we must ask whether the condition of the left knee-jerk in this patient possibly puts the entire diagnosis of functional paralysis in peril. But here, as indeed in all clinical problems, we must include all the facts within our review. We have seen that there is an extremely strong body of evidence in support of a functional interpretation of the case, and the question now is whether this one opposing condition, namely a defective knee-jerk, is sufficient to neutralize this evidence. Personally I do not think so. Of course, it must be allowed that functional nervous disease usually produces exaggerated tendon phenomena, and this is illustrated by the heightened right knee-jerk in the present patient. But when, as over this patient's left side, there is profound anæsthesia, of the deep as well as of the superficial tissues, it is hardly surprising that the knee-jerk is defective, seeing that the stimulus which is used to provoke it is here applied to a non-sensitive surface. At all events, there is the fact and such explanation of it as seems not unreasonable.

Again, in this association the condition of the plantar reflex is obviously of importance. An extensor response would, in the circumstances, make organic disease almost, if not absolutely, a certainty. But there is no extensor response, either on the one side or the other. On the contrary, there is in the right foot a distinct flexor response, while from the left the response is absent—that is, so far at least as the left foot is concerned, the condition is the one common in functional nervous disease, and the contrast between the two sides emphasizes this explanation as applicable to the present case.

In view of all the facts, therefore, neither the facial paralysis, nor the ankle-clonus, nor the defect of the left knee-jerk, ought, singly or in combination, to be allowed to qualify the diagnosis in this case as one of functional nervous disease producing hemiplegia, hemianæsthesia, hemianalgesia, and crossed amblyopia.

With regard to treatment, doubtless the best course, had it been possible, would have been to have placed the patient in a nursing home, excluded his friends, and brought him under the influence of isolation, massage, electricity, abundant food, and the confident expectation of recovery. We cannot obtain all these advantages in hospital, but we have so far done our best, and with not unsatisfactory results. At one time the paralysis of the left limbs was almost absolute. Now he can move his upper limb freely, and can walk without assistance. Even in his partial recovery he contributes something positive to the diagnosis, for his gait is not that of the ordinary hemiplegic; the left limb is not, as in the more familiar cases, swung round or circumducted as it is advanced, but the foot is rather dragged along the ground, so that it always appears to be lagging behind its fellow. Again, there is no rigidity in the paretic limbs, such as would almost certainly be the case in a hemiplegia dependent upon organic disease and of more than a year's duration. The character of the gait, and the absence of rigidity, in short, favour the view that the case is one of functional disturbance, not of organic disease.

As regards the future, it may be expected that progress will continue, and probably, when the dispute arising from the original accident is settled, and the patient receives a financial *solatium* for all his troubles, there may be a more rapid movement towards recovery. This is not to suggest that the patient, even in the slightest degree, is malingering. On the contrary, he is the subject of a very real disease, and one penalty of a failure to appreciate this, is inability to cure these cases. With the suspicion in his mind that his medical adviser believes him to be shamming, the patient will certainly not make progress, and if he is well advised, he will, in such circumstances, promptly transfer himself to the care of some more enlightened physician. On the whole, however, the principal importance of the case is the completeness with which it illustrates many of the possible events among the more severe manifestations of functional nervous disease, and as a problem in diagnosis it is well worthy of careful study.

A CLINICAL LECTURE ON SOME CASES OF INFECTIVE ENDOCARDITIS.

Gentlemen,—The case which is to form the principal text for our discussion this afternoon is that of a married woman, aged 29, who unfortunately died on the twelfth day after admission to hospital, and in whom the clinical picture, at least in all its details, was by no means easy to read. At the outset, it appears, the patient complained solely of pain in the right foot; and this, it was said, had been troublesome for ten or twelve days and had been attended by some degree of fever and by a general sense of ill-health. When received at the hospital, and in view of the apparently local nature of the complaint, the woman was at first admitted to a surgical ward, and there she remained for eight days. The temperature during this time was irregularly febrile (Chart 1), and there were at times some rather free sweatings, but no adequate explanation of these facts could be obtained. All that could be said of the painful joint was that there was tenderness on pressure over the ankle, and a degree of discoloration or ecchymosis of the skin on the dorsum of the foot. Manifestly these local facts were hardly sufficient to account for the temperature disturbance, but, as already said, no further explanation could be found; and, in particular, an examination of the pelvic organs disclosed nothing to suggest the diagnosis of septic or gonorrhœal infection —a possibility which has always to be reviewed in cases of arthritis for which no obvious interpretation is to hand. Indeed, the limitation of the arthritis, as in the present instance, to one or two joints, is an arrangement by no means infrequent in cases of so-called gonorrhœal rheumatism. With a negative pelvic examination, however, such a diagnosis could not be sustained, and thus for some eight days the case remained without any very confident label. At the end of this time a sudden and striking change took place. The patient, who previously had seemed a calm, rational, and self-possessed woman, suddenly commenced to scream and to talk incoherently; she vomited, passed urine involuntarily, and evidently was entirely oblivious of her surroundings.

This was an unexpected development, and withal a somewhat puzzling one. It was certainly not to be explained as the result of febrile disturbance, for, as a matter of fact, the temperature had declined, and on the day of the seizure the record had even been subnormal. Nor could hysteria be accepted as a probable interpretation; other facts apart, the incontinence of urine and the earlier history of the case opposed such a view. Hence it seemed necessary to admit that some serious intracranial mischief was responsible for the new aspect which the case had assumed; but the nature of this mischief, and its relation to the previous temperature record and to the pain in the right foot, were clinical problems not readily solved.

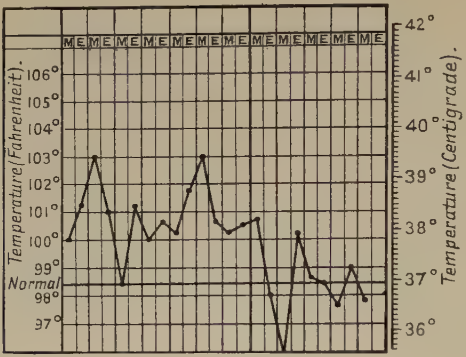


CHART I.

With the suspicion just mentioned in view, a careful re-examination of the nervous system was now made.

The patient was obviously unconscious, and there was a good deal of restless tossing of the head and limbs, but beyond this, no evidence of organic nervous disease was discovered—with one important exception, namely, a quick extensor response of the great toe on each side. In these circumstances, meningitis seemed at least a possibility, though Kernig's sign was not present and the fundus oculi on each side was normal. Lumbar puncture was therefore performed. Again, however, there was no help towards a positive diagnosis, for the cerebro-spinal fluid flowed freely and naturally; further, it was quite clear and there was no excess of cellular elements. [Later, Dr. Torrens was able to state that it contained no micro-organisms.] Thus, so far as the nervous system was concerned, we were left with one positive organic sign, namely, a bilateral extensor response, *plus* the mental condition already described.

There were, however, certain other facts discovered, and some of these were of manifest importance. In the first place the spleen was enlarged; secondly, no pulse could be felt in the right radial artery; thirdly, there was a slight amount of albumin in the urine; and fourthly, Dr. Harwood, who examined the blood, reported a definite leucocytosis.

But attached to some of these facts there were certain difficulties or qualifications. For example, it was not certain that the splenic enlargement was a new or recent event, and now and again one does find a spleen palpable below the hypochondriac margin and yet without any very precise clinical significance; and as the patient here was unconscious it was impossible to determine whether the spleen was or was not tender on pressure. Similarly, the albuminuria, it was recognized, might be explained by the febrile or other disturbance, and was therefore not entitled to be endowed with any very exact meaning. At first sight the absence of the right radial pulse appeared very confidently to be both a new and an abnormal fact; yet here again there was some difficulty, as it appeared that all previous observations on the pulse had, by a curious coincidence, been taken at the *left* wrist. A little investigation, however, showed that the absence of the right arterial pulse affected not only the radial artery, but also the ulnar, and further, that no pulsation could be felt along the whole course of the right brachial, the arterial pulse in the left upper limb and elsewhere being apparently normal. In these circumstances the suggestion of some anatomical peculiarity as the cause of the defect in the right radial pulse could not be regarded as tenable, and the absence of pulsation from the arteries of the right upper limb had therefore to be recognized as a definitely pathological condition, and one probably of recent origin. Whatever therefore might be said of the splenic enlargement and of the albuminuria, there remained the arterial obstruction, the leucocytosis, the unconsciousness, the bilateral extensor response, and the somewhat indefinite condition of the right ankle joint, as a body of abnormal facts demanding inclusion in a comprehensive diagnosis.

Now manifestly there is no acute cerebral event, such, for example, as meningitis, which will explain all these facts—certainly not the loss of the pulse in the right upper limb. The same may be said of intracranial tumour and of cerebral hæmorrhage and thrombosis. And enteric fever, which may well be attended by delirium and by enlargement of the spleen, does not cause either an extensor response, or loss of the radial pulse, or a leucocytosis—indeed, this disease is marked by a diminution rather than by an excess of the white cells of the blood.

Another suggestion advanced to explain the cerebral disturbance was embolism, but here also there were difficulties. No source of origin of an embolus could be discovered. The patient, it was true, had had two attacks of "rheumatism" in early life, but there were no physical evidences of cardiac valvular disease; the cardiac sounds, indeed, as described in the hospital record, were "entirely pure and beyond suspicion," and the præcordial dulness was normal.

Moreover, cerebral embolism does not usually produce mental excitement, but rather paralysis, as hemiplegia, and perhaps unconsciousness; and undoubtedly, cerebral embolism could not account for all the other clinical aspects of the case. Hence, while it seemed certain that some acute pathological condition existed within the skull, it was difficult, so long at least as attention was confined to the evidences of disturbance of the nervous system, to be quite sure of the nature of this condition. In these circumstances, it was manifestly advisable to fall back on some less equivocal feature of the case, and to endeavour to develop a diagnostic advance from a new starting-point. And for this purpose the absence of the pulse from the arteries of the right upper limb was obviously suitable, as the possible explanations of such a defect fall within an exceedingly narrow compass.

There was not the slightest reason to believe that the absence of pulse was here due either to an aneurism or to any unusual condition causing pressure on the artery from the outside, and therefore the block, it was realized, must be due to internal obstruction, and for this only two possibilities presented themselves, viz., thrombosis and embolism. But arterial thrombosis, save in vessels the subject of senile or other form of degeneration, is a very exceptional event; though it has been known to occur, even in young adults, as a consequence of chlorosis and during convalescence from influenza and enteric fever. Hence, and especially as none of the conditions just mentioned existed, it seemed a safe conclusion to put thrombosis aside and to regard our patient as the subject of embolism either of the upper part of the brachial or of the axillary artery. It is quite true that the limb was not paralysed, or at least that it was tossed restlessly about, as were the other limbs, and that, except for the absence of pulse, there were no definite evidences of disturbance of the circulation in the limb. But, though embolism of an artery in one or other of the limbs sometimes causes such symptoms as paralysis, pallor and coldness of the skin, and even œdema and gangrene, it does not always do so. Indeed, however the fact is to be explained, it is remarkable how slight may be the clinical disturbances which attend such arterial obstruction, and this is one reason for cultivating the habit of examining the pulse, not only in the radials, but also in other readily accessible arteries.

When this practice was adopted in the patient under consideration, no further evidence in support of the suggestion of embolism could be secured; alike in the left upper limb and in both lower limbs, each of the arteries within reach of palpation betrayed an easily appreciated pulse. Yet, for the reasons already given, we held fast to the view that an embolus was responsible for the arterial obstruction in the right upper limb, and this once granted, several other facts in the case became susceptible of a fairly confident explanation on similar lines. Splenic enlargement (often with localized tenderness, and sometimes with peritoneal friction) is a recognized result of embolism of the spleen; albuminuria (sometimes, but by no means invariably, accompanied by hæmaturia) may be an expression of renal embolism; pain, tenderness, and sometimes effusion, in one or more joints, may also be due to embolism; and when with these events (more or less), and with obstruction of the brachial artery, there was some active disturbance of the central nervous system—unconsciousness, excitement, extensor response—for which no alternative interpretation could be obtained, it seemed necessary to admit that this disturbance also, although not following the common type, might reasonably be regarded as dependent on embolism. Thus, with no great strain of argument, all the prominent clinical facts of the case seemed to find in multiple embolism a fairly complete and convincing explanation.

When, however, this position was reached, there immediately recurred the difficulty of finding a site of origin for the emboli, for, as every schoolboy knows, coagulation does not occur in the arterial circulation save only, at least with rare exceptions, when the blood is in contact with a diseased or injured cardiac or vascular surface. And while a single artery, or perhaps neighbouring arteries, may be blocked by a thrombus displaced from a larger arterial trunk or from an aneurism, the existence, as indicated in this case, of emboli in many different parts (spleen, kidney, brain, and limbs) involves the conclusion that such emboli must have been distributed from the heart. Hence, as the cardiac condition in the patient, so far at least as physical examination could determine it, was normal, there seemed at this juncture to be a definite danger that an interpretation of the case based upon the suggested existence of multiple emboli, must needs be brought to an abrupt and compendious end. The entire scheme of diagnosis, in short, appeared to be in peril.

Quite reasonably the question might have been put: How is it possible to hold that this patient is the subject of multiple emboli in the systemic circulation when there is not the slightest evidence of any disease of the left heart, whence alone such emboli could take origin? And if the present case stood alone, such a question might well constitute a serious objection, or indeed a fatal objection, to the diagnosis here proposed. But it is certain, explain it how you will, that in the most serious form of endocarditis—the so-called infective or septic endocarditis—there may be a deposit of fibrinous masses, even on a relatively large scale, either on the cardiac valves or at other points on the inner surface of the chambers of the heart, and yet the cardiac physical examination may be perfectly normal. Guided by experiences of this order, we therefore adhered to our diagnosis of multiple embolisms, and ventured to attribute these to a process of septic endocarditis affecting the left heart. The specimen (fig. 39) affords a full justification of this position, for it shows an enormous clot adherent to the upper or auricular surface of the posterior cusp of the mitral valve. For the rest, the post mortem examination detected an embolus blocking the third part of the right axillary artery, and another in a branch of the right middle cerebral artery; and both in the spleen and in each of the kidneys were numerous evidences of embolism in the shape of recent infarcts.

Hence it is evident that a diagnosis of septic endocarditis may be justified even when the heart to physical examination seems normal; and indeed, it may be said that when there are evidences of emboli in many parts, together with a febrile temperature, such a diagnosis is demanded whether direct evidences of cardiac disease do or do not exist. A simple endocarditis, on the other hand, apart from active evidences of rheumatism, does not cause fever, and although it may give rise to embolism, this, as a rule, is limited to one, or perhaps to two organs, and has not the widespread distribution which was a prominent feature of this particular case.

This possible absence, then, of all physical evidences of cardiac disease in cases of septic endocarditis, is the prominent clinical lesson of the present record. To emphasize this lesson, I now show a second specimen (fig. 40), in which is seen a large thrombus attached to one of the curtains of the mitral valve and hanging downwards into the left ventricle. Here, also, there was no cardiac murmur, and the physical signs were limited to those of pleural effusion and of splenic enlargement and tenderness; and with these were several weeks of high temperatures.

Such cases illustrate one of the difficulties which may attend the diagnosis of infective endocarditis. They show, also, how significant may be the clinical meaning of multiple embolism in association with febrile disturbance—and this even in the absence of direct evidence of cardiac valvular disease. It was on such an association, together with a blood examination showing a leucocytosis, that the diagnosis in our first case was made; there was no cardiac murmur, and no micro-organisms were discovered in the blood, and both of these negatives have to be recognized as possibilities in the diagnosis of septic endocarditis. There is another negative which also is not infrequent and which the present case illustrates, namely, the absence of any recognized source or focus from which the general infection may have taken origin; even after the post mortem examination we are still ignorant of the avenue through which infection occurred. No cardiac murmur, no micro-organisms found in the blood, and no recognized source of infection: and yet a fairly confident diagnosis of infective endocarditis based upon the evidence of multiple emboli in association with pyrexia and leucocytosis—such is a succinct clinical summary of the present record.

There are, in addition, one or two details worthy of attention. First we may revert for a moment to the nervous symptoms, and especially to the mental disturbance and to the existence of a bilateral extensor response. In view of the sudden development of these symptoms, and of the subsequent discovery of an embolus in one of the smaller branches of the right middle cerebral artery, it seems necessary to admit that embolism was in some fashion or other the cause of the symptoms. Yet there was no post mortem evidence either of meningitis or of cerebral softening, and therefore the probability would appear to be that the embolus produced the condition of unconsciousness by toxic rather than by inflammatory or mechanical influences.

But what shall be said of the *bilateral* extensor response, in view of the *unilateral* cerebral lesion? There can be no doubt that an extensor response on one side may be the first, and even for some time the only clinical sign of a lesion, perhaps a hæmorrhage or thrombosis, in one pyramidal tract; so that *a priori* a bilateral extensor response ought to mean a lesion of both pyramidal tracts. And, without doubt, this interpretation is sometimes the correct one. Yet when the patient is unconscious it may be doubted whether such an interpretation is always justified. During sleep, even in perfectly healthy persons, an extensor response may sometimes be obtained, and in view of this it is not hard to believe that the same result may accompany a state of unconsciousness, and this whether such unconsciousness is produced by a brain lesion or by some other mode. At all events, here is a case in which, the patient being unconscious, a cerebral lesion of strictly unilateral distribution was associated with a bilateral extensor response; and perhaps some support for the suggestion just advanced may be found in the fact that this response existed apart from other evidences of a lesion of the pyramidal tracts, namely, exaggeration of the knee-jerks and ankle clonus—the knee-jerks were well within normal limits and no clonus could be obtained.

Next may be added a word on the condition of the ankle-joint, for which, in the main, the patient obtained admission to hospital. As already described, the evidences of physical change in the joint were very slight, and possibly they depended on toxic irritation rather than on embolism. There may, however, have been minute emboli in the vessels of the joint—and the discoloration of the skin suggests this—as it is known that in infective endocarditis one or more joints may be affected in this fashion, and thus an acute arthritis, and even suppuration in the joint, may be produced.

The record of this first case, then, in addition to the difficulties of diagnosis already discussed, shows interesting features in reference to the patient's mental condition, to the state of the plantar reflexes, and to the examination of the ankle-joint.

I have here another patient in whom, possibly, the question of a diagnosis of infective endocarditis might be raised. She is a woman aged 48, and she states that for some years she has been the subject of "heart disease"; and as there is considerable hypertrophy of the left ventricle her story may be accepted as true. About twelve months ago she noticed a peculiar sound, apparently in her neck or chest, when she was lying quietly in bed, and it was this which brought her to the hospital. On examination you will at once detect an aortic diastolic murmur having a decidedly musical quality, and there can be no doubt that this is the sound which has attracted her attention. What makes her presentation appropriate on the present occasion is, that given other suspicious circumstances, a decided change in the quality of an existing cardiac murmur, or the development of an additional murmur, may well be urged in support of an opinion that the responsible endocarditis is, not simple, but septic or malignant. The rapid and extensive formation of fibrinous deposits which characterizes infective, as compared with simple endocarditis, readily explains why rapid changes in the auscultatory examination of the heart may occur in the infective form of the disease, and why importance should be attached to such changes. But the development, even the sudden development, of a musical quality in a previously existing cardiac murmur cannot be claimed as *conclusive* evidence of the infective nature of an endocarditis, as this woman has been under close observation for a period of nine months, entirely without any embolic manifestations, and with only those limitations of good health which are common in the subjects of aortic regurgitation.

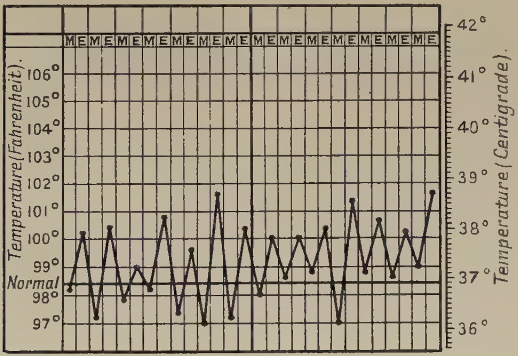


CHART 2.—Part of a chart from a case of chronic infective endocarditis. The temperature continued after the fashion here shown for at least thirteen weeks. There were no physical signs other than a mitral systolic murmur.

There is still another clinical association of the cardiac condition in infective endocarditis which is worthy of note, and I am able to illustrate it by the charts of two patients recently under observation (Charts 2 and 3). As we have already seen, the disease may exist without local evidence of cardiac disease; further, a decided change in an existing cardiac murmur may be a suspicious or suggestive fact. Now

I add that a further contribution towards the same conclusion is afforded by the development of unexplained pyrexia in a patient known to be the subject of chronic endocarditis. Such a combination is in the highest degree suspicious. It demands the most careful investigation with a view to determine the existence and nature of any general infection that may be present; and even when, as sometimes happens, this investigation yields only negative results, the continuation of the fever, without any available interpretation, makes a diagnosis of infective endocarditis fairly certain. In some of these cases the temperature, without perhaps being very conspicuous, is found, at some time or other within the twenty-four hours, to be definitely above normal, and this may be continued for weeks or months. It is quite a mistake to suppose that septic endocarditis is in all cases a disease of an extremely acute or urgent character. Here (Chart 2) is a fair specimen taken from a record which extended over thirteen weeks, the patient, except for a gradual decline in strength, showing no evidences of disease other than a mitral systolic murmur and the abnormal temperature; embolic phenomena were entirely absent.

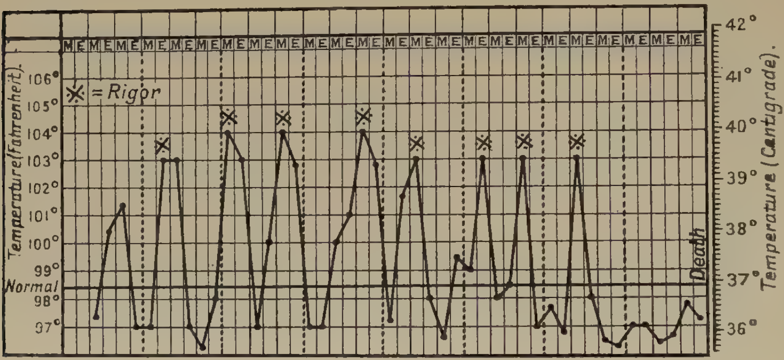


CHART 3.—Intermittent type of temperature in infective endocarditis.

In Chart 3 the fever is a much more obtrusive fact, and the chart illustrates well the intermittent type of pyrexia which is sometimes prominent in infective endocarditis, and which may be attended, as here, with such phenomena as rigors and sweating, so that the case at first sight may resemble malarial fever. Here also, at first, the clinical facts were restricted to the pyrexial disturbance *plus* systolic and diastolic aortic murmurs; later there were such evidences of embolism as hæmaturia, epistaxis, and hæmatemesis. The points, however, which these two cases particularly enforce are, first, that the appearance of unexplained pyrexia in a patient known to be the subject of simple endocarditis is an anxious event; and, secondly, that the persistence of such pyrexia, even without embolic or other evidences, points more and more confidently to a diagnosis of infective endocarditis. These doctrines are illustrated in the two instances just quoted, while the first two cases I have described afford examples of the diagnosis of infective endocarditis in the absence of physical signs of heart disease, and the third shows that an alteration in the physical quality of a cardiac murmur, which, in the presence of pyrexia or other circumstances, might well be suspicious of the more serious diagnosis, is yet, at least at times, not inconsistent with a simple, as distinct from an infective, or, as it is occasionally termed, a malignant endocarditis.

As in none of the cases here quoted was treatment successful, the records do not, in themselves, afford much opportunity for practical comment in this direction. But it may be said generally that if the nature of the micro-organism responsible for the condition can be determined, there is some chance that the infective process may be brought to an end by the use of an appropriate serum or vaccine. In individual cases the disease has been shown to be due to the pneumonia diplococcus, to the gonococcus, or to the bacillus of enteric fever or diphtheria; and such cases have been successfully treated. But most cases of infective endocarditis are either streptococcal or staphylococcal infections, and, probably, when the nature of the infection cannot be determined, the best plan is to use a polyvalent antistreptococcic serum, which, so far as my own experience goes, is quite as effective when given *per rectum* as it is when given hypodermically. Sometimes the serum produces very striking results, but it is to be feared that the majority of cases of infective endocarditis present features which are beyond the reach of any remedy at present at our disposal.

A CLINICAL LECTURE ON THE DIAGNOSIS IN A CASE OF PERNICIOUS ANÆMIA.

Gentlemen,—The patient to whose case I wish to direct your attention this afternoon is a woman aged 52. Some fifteen to eighteen months ago she began to complain of general weakness and of breathlessness on exertion, and these symptoms have gradually become more troublesome. For several months, too, there has been œdematous swelling of the lower limbs, and neither tonics, given with the view of improving the general health, nor diuretics, ordered in the hope of removing the dropsy, have produced any substantial benefit. The œdema has, I understand, been considered a rather disconcerting and mysterious occurrence, seeing that no organic disease of any of the thoracic or abdominal organs, and competent to explain the existence of the dropsy, can be discovered. Thus the case has been regarded as somewhat of a diagnostic puzzle, inasmuch as, while œdema of the lower limbs is a conspicuous symptom, none of the usual causes of this condition appears to be present. The fact is, I suppose, that dropsy of the lower limbs is so frequently the result of disease of the heart, kidneys, or other organs, that its existence at once arouses suspicion in these directions, and when physical examination in an individual case shows this suspicion to be baseless, there is apt to be a sense of surprise. The surprise, however, ought only to be momentary, as there are well-established causes of œdema of the lower limbs other than organic affections of certain of the thoracic and abdominal viscera. Thus some of you may recall a series of cases of peripheral neuritis in chronic alcoholic patients, and attended by œdema of the feet and legs, which I demonstrated in this classroom last year. The suggestion in such cases is, that the poison, by paralysing the vaso-motor nerves, upsets the balance of the tissue circulation and so favours the accumulation of fluid in the tissue spaces. Whatever be the exact explanation, the fact is beyond dispute, that peripheral neuritis, as well as renal and cardiac disease, may be associated with subcutaneous œdema.

Another cause of such œdema is found in various diseases which depreciate the quality of the blood. Here there is probably both malnutrition of the capillary blood-vessels, allowing increased transudation of fluid, and malnutrition of the cardiac muscle, with, as a consequence, a sluggish circulation in the capillaries and veins; and both these agencies will be factors in the production of the dropsy. Examples of the action of defective blood-quality as a clinical cause of œdema are sometimes seen in chlorosis and in the cachexia produced by malignant disease. But probably the most severe manifestations of œdema due to this cause occur in such blood diseases as pernicious anæmia, leucocythæmia, and other conditions of a more or less similar nature. Therefore, both peripheral neuritis and various abnormal blood conditions must be numbered among the possible causes of œdema of the lower limbs, and when, as in the present instance, clinical examination shows, together with such œdema, the absence of visceral disease, these causes ought to come prominently into view. Had this truth been borne in mind, it is probable that the correct diagnosis in the present case would have been reached at a much earlier stage. For, just as examination of the patient shows the absence of all distinctive evidence of visceral disease, it also shows the absence of all signs of peripheral neuritis. Hence, by the method of exclusion, all positive facts apart, the inquiry is naturally directed to the condition of the blood as the probable source where an explanation of the œdema is to be found.

Another aid to clinical investigation which might with advantage have been laid under contribution in the present patient at an earlier stage is the ophthalmoscope. Immediately this method of observation is employed, the diagnostic position of the case is greatly improved. For, scattered widely over each fundus oculi are numerous retinal hæmorrhages, and multiple retinal hæmorrhages having a bilateral distribution carry a fairly precise significance, and this more particularly when, as here, renal disease and diabetes can be excluded from the diagnosis. In such circumstances the suggestion of some serious blood disease is most direct and strong, and of these pernicious anæmia is perhaps the most frequent, and certainly is the one most apt to be overlooked. It may, indeed, be fairly said, that in practical experience, and given the absence of renal disease and of diabetes, the recognition of multiple retinal hæmorrhages in each fundus oculi is most frequently associated with pernicious anæmia. The enlargement of the spleen or lymphatic glands, or of both of these, in other blood diseases, such as leucocythæmia and Hodgkin's disease, makes an error in diagnosis in such conditions improbable. But in pernicious anæmia, on the contrary, the gradual movement of the disease and the absence of physical signs, using this term in its conventional sense, are apt to mislead the observer and to cause the diagnosis to be missed. Hence the great advantage of recognizing that in this disease multiple retinal hæmorrhages are of frequent occurrence. It cannot be claimed that they are present in every case, but they are a prominent fact in the great majority of cases, and were the ophthalmoscope used as a routine procedure the disease would most certainly be less frequently overlooked. No doubt, as already stated, retinal hæmorrhages occur in renal disease, in diabetes, and in such blood affections as leucocythæmia, etc. But these conditions are not likely to be confused with pernicious anæmia by any careful observer, and, given proof of their absence, the existence of retinal hæmorrhages raises the probability of pernicious anæmia to a very high level.

Further, the retinal hæmorrhages of pernicious anæmia afford assistance in distinguishing this disease from certain other conditions with which it is more or less apt to be confused. One of these is malignant disease, another, severe chlorosis, and a third, anæmia secondary to loss of blood. Not a few cases of malignant disease, and more particularly of malignant disease of the stomach, have undoubtedly been regarded as examples of pernicious anæmia until the discovery of a tumour, either later in the history of the case or on post mortem examination, has demonstrated the correct diagnosis. Indeed, though the statement is rather of the rough-and-ready order, it is not without value to repeat that when a patient is manifestly the subject of some serious illness having a more or less chronic course, extending, say, over several weeks or months, and not explained by evidences of visceral disease, the probability either of pernicious anæmia or of malignant disease should be carefully remembered. The point of special interest here is, that in the circumstances just related, the existence of multiple retinal hæmorrhages, as in our present patient, strongly inclines the diagnosis in the direction of pernicious anæmia. With regard to chlorosis and to the anæmia following hæmorrhage, it may be accepted that in these conditions retinal hæmorrhages are exceptional, and that even when present they are generally both few in number and insignificant in size. Thus the position is reached that retinal hæmorrhages occurring in association with such general symptoms as anæmia, progressive weakness, and breathlessness, and apart from renal disease, from diabetes, and from enlargements of the spleen and lymphatic glands, are usually, if not invariably, an evidence of pernicious anæmia. Malaria may perhaps be mentioned as a general disease in which multiple retinal hæmorrhages are occasionally seen, but cases of malaria are infrequent in this country, and the clinical history and examination of the blood are sufficient, other facts apart, to establish the diagnosis.

Here, then, is a patient who is presented as the subject of an œdema existing apart from visceral disease. Such a conjunction of events has compelled us to consider the possibility (1) of peripheral neuritis, and (2) of some serious depreciation of the quality of the blood. Of the first of these there is no evidence—no anæsthesia, no muscular weakness or atrophy, no tenderness of the muscles or nerve trunks, no ataxia, and no deficiency of the knee-jerks. On the other hand, the patient is certainly anæmic, and ophthalmoscopic examination displays a picture which, in the circumstances, is in the highest degree suggestive of a recognized blood disease, namely, pernicious anæmia.

Having reached the position just described, the next step in the investigation of the case was naturally an examination of the blood, and when this was undertaken, all possibility of doubt disappeared. The red corpuscles were found to be reduced to 31 per cent. of the normal, and the hæmoglobin to 41 per cent., the greater reduction of the corpuscles than of the hæmoglobin indicating a high colour index (1.3), and being, as is well known, a characteristic feature of the pernicious type of anæmia. In addition, the red cells varied much in size, many were conspicuously deformed or altered in shape (poikilocytosis), and a few nucleated red cells were seen. The white corpuscles numbered only 1,200 per cubic mm.—that is, they were far below the normal count, and this condition of leucopenia, in association with a marked reduction of the red cells, with the presence of nucleated reds, and with a high colour index, is highly suggestive of pernicious anæmia.

It is not unimportant to state the position in the fashion just adopted, for the blood examination should never be exalted to an absolutely conclusive position. With such a combination of blood conditions as exists in the present case the suggestion of pernicious anæmia certainly reaches a very high level. But the level is hardly one of absolute certainty. And, on the other hand, it must be remembered that in some stages and phases of pernicious anæmia the blood may give no positive evidence to support the diagnosis. All the various aspects of the case, and not merely the blood examination, must be brought under review, and in particular it is necessary to make more than one examination of the blood, and this especially when the first examination gives only negative results. Probably too much stress is often placed on the condition of the blood in the diagnosis of pernicious anæmia, and it is well to emphasize the necessity of keeping the facts so obtained in their proper position and due perspective.

Reviewing then the present case, it may be said that its special interest resides in the experience (1) that its leading clinical feature, namely, œdema of the lower limbs, is discovered on examination to be an expression of pernicious anæmia; and (2) that the œdema was conspicuous at so early a date in the history of the case that the existence of anæmia as its cause was overlooked. This is by no means an exceptional experience, and hence it is well to take note of the practical lesson which it illustrates.

There is another feature of the clinical record which also is of some considerable diagnostic importance—namely, a certain degree of pyrexia. When first seen in the out-patient department the patient was found to have a temperature of 100.6° F., and though within a few days after her admission to the hospital the temperature had fallen to the normal level, there have been on several occasions during her residence records approaching or reaching 100° F. Now this is a common experience both in pernicious anæmia and in certain other affections of the blood, and it is of distinct value in establishing the diagnosis. Occasionally, too, the temperature in pernicious anæmia is a commanding feature of the case. It may reach 103° or 104° F., or even higher, and exceptionally these high records continue more or less persistently for several weeks. In this is to be found an explanation of the ease with which now and again a case of pernicious anæmia receives a diagnosis of enteric fever; and the mistake is the more readily made when, as is by no means infrequent, diarrhœa and enlargement of the spleen are numbered among the clinical evidences of the disease. A febrile temperature without physical evidences of any disease competent to explain it, is one of the frequent problems of medical practice. Such an experience necessarily gives rise to suspicions of enteric fever, of tuberculosis, or of some form of septicæmia. It is well to add pernicious anæmia and other more or less closely allied disorders of the blood to this list of the possible causes of pyrexia existing apart from physical signs of disease. And when this is remembered, a certain number of cases of "mysterious" pyrexia will be readily and accurately interpreted. Unlike the œdema, the existence of pyrexia can hardly be regarded as directly due to the anæmia. Its existence in our present patient, and to a still more marked degree in many other patients, goes therefore to support the view that pernicious anæmia is a disease due to some toxic agent exerting a prejudicial influence, not merely on the blood, but on many other tissues. Hence many of the phenomena of the disease—for example, occasional or persisting pyrexia, vomiting, diarrhœa, enlargement of the spleen—which are difficult to explain as consequences of anæmia, become readily understood; they appear, equally with the anæmia, as direct and immediate effects of the primary toxic agent—an agent which, according to Dr. Wm. Hunter, is one possessing distinctive and specific characters.

The suggestion or supposition that pernicious anæmia is essentially a toxæmia, has, of course, important applications in reference to the treatment of the disease, but for the moment I wish rather to emphasize its relation to diagnosis. Manifestly, on such a view, it is conceivable that while in one case the blood may suffer the main damage—in other cases the stress of the disease may fall on other tissues, with the result that vomiting, diarrhœa, temperature disturbance, splenic enlargement, etc., may be the most conspicuous clinical features. In such circumstances the physician's attention is, as it were, directed away from the anæmia—which, by the way, is not invariably and in all stages of the disease of an extreme type—and thus the true nature of the case may be missed. Hence the fact of pyrexia in our present patient not only helps the diagnosis in this individual instance, but serves also to press on our attention the more modern view of the pathology of pernicious anæmia. And this view, leaving for the moment the question of its accuracy on one side, has the practical advantage that it prepares us to find in cases of pernicious anæmia signs and symptoms other than those which are merely secondary to the state of anæmia; it also lessens the risk of an error in diagnosis, when, as sometimes happens, such signs and symptoms form the prominent clinical features of the case. In our present patient, the œdema, and to some extent the pyrexia, seem mainly to have attracted attention, and to have obscured, though perhaps not quite justifiably, the importance of the anæmia; in other instances, marked and persisting temperatures or severe gastro-intestinal symptoms may exercise a similar misleading influence.

There is yet one further group of symptoms which may be present in pernicious anæmia and which, by their relative prominence, may cause the true diagnosis to be overlooked. I refer to symptoms of nervous disturbance. It is well known that pernicious anæmia is in occasional instances associated with a combined sclerosis of the posterior and lateral columns of the spinal cord, and there is evidence also that it may be associated with peripheral neuritis. Hence a case of pernicious anæmia may appear with definite signs of organic nervous disease; and here again, unless the practitioner is alive to the possibilities of the situation, the case may be seriously misread. But rather than these comparatively conspicuous changes, I wish to emphasize the commanding position which pain, and especially abdominal pain, may occupy in the symptomatology of pernicious anæmia. This point is not illustrated in our present patient, but it may be regarded as appropriate to our discussion as it is one of the events responsible at times for failure to recognize a case of pernicious anæmia. The pain is usually at the epigastrium, but it may extend over the whole abdomen, and more particularly when, as is not uncommon, vomiting is a feature in the case, the condition is apt to be mistaken for some form of dyspepsia; in occasional instances hæmatemesis occurs, and thus provides a further opportunity for misreading the diagnosis. Still another feature of pernicious anæmia which may lead to a suspicion of serious disease of the stomach is the absence of hydrochloric acid from the gastric juice. For the most part the abdominal pain or other form of discomfort is far from being severe, but in other cases it is extreme, and may even be described as agonizing. In one instance I have known the abdomen to be opened on the view, suggested by paroxysmal attacks of severe pain, that the case was probably one of biliary or pancreatic calculus. Dr. Hunter speaks of "acute gastric discomfort" and of "attacks of tearing, griping pain in the intestine," and Dr. J. George Taylor has described an instance in which the pain "came on rather suddenly, lasted for several hours, and appeared to cause intense agony." Therefore abdominal pain, even of a severe order, must be included among the possible events which find an explanation in the diagnosis of pernicious anæmia.

To sum up the clinical experience of this individual case, the problem was to find an explanation of a considerable and long-standing œdema of the lower limbs. Examination of the chest, abdomen, pelvis, and urine proving negative, the possibility of peripheral neuritis or of some disease of the blood came into view. As the usual tests of the nervous apparatus gave normal results, peripheral neuritis was excluded, and the condition of the blood was brought acutely under suspicion. This suspicion was deepened by the discovery of multiple retinal hæmorrhages, and was confirmed by an examination of the blood itself. Thus the case is correctly presented as one of pernicious anæmia associated with considerable subcutaneous œdema, retinal hæmorrhages, and a moderate degree of pyrexia. And perhaps its principal value as a clinical study is the illustration it affords of one of the fashions in which, in consequence of the prominence assumed by an individual symptom, the anæmia may be, as it were, pushed into the background, and may thus fail to receive in adequate measure the attention of the clinical observer.

A CLINICAL LECTURE ON A CASE OF CANCER OF THE LIVER.

GENTLEMEN,—The case to which I wish to direct your attention to-day is that of the man, J. N—, aged 48, who was admitted to hospital on November 22 and died on December 5. You will remember that our clinical observations failed to determine a completely satisfactory explanation of the case, but now that the results of the post mortem examination are before us, we are provided with a solution of our difficulties.

The patient, as we saw him on admission, appeared fairly strong and well nourished. He complained of a certain amount of pain in his left side, of some discomfort after food, and of slight loss of flesh; and he had been conscious of these symptoms for a period of six months. At no time, however, had he vomited or regarded himself as in any way seriously ill. Indeed, he had continued his occupation as an ironfitter until the day of admission, and it was only on the urgent representations of a medical man who had examined him in connection with his complaint of dyspepsia, that he consented to come into hospital.

Physical examination at once detected very manifest evidences of serious disease. There was, in the first place, a tumour in the left hypochondrium, which we regarded with great confidence—though, as the sequel will show, incorrectly—as an enlarged spleen. It occupied exactly the position of a splenic tumour; was influenced by the descent of the diaphragm; was freely movable on bimanual palpation; and was dull to percussion. No notch could be detected, but this, though a valuable sign when present, is by no means always to be recognized even in considerable splenic enlargements. In the second place, there was extension downwards of the area of hepatic resistance and percussion, so that the lower border of the liver in the middle line was placed only about 1½ inches above the umbilicus; the projection downwards to the right of the middle line was rather less decided, but even here the liver could be readily felt for some distance below the costal margin. There was also a distinct "lump," about the size of a small Tangerine orange and situated close to the hypochondriac border in the line of the right nipple. This "lump" moved with the excursion of the diaphragm, and could not be separated from the liver either by palpation or percussion. We had, therefore, little doubt that it was a projection from the upper or convex surface of the liver. The exact nature of the "lump" was much more uncertain. There was no sense of fluctuation, such as we should expect in a projecting hepatic abscess, nor did it give the fremitus which is sometimes obtained on percussing a hydatid cyst. It was, indeed, quite hard and resistant; and both it and the liver generally could be palpated without causing the patient any pain or suffering. These latter facts seemed decidedly against the suggestion of malignant disease of the liver, in which the nodules, when present, are usually both painful and tender. The absence of any well-defined or irregular lower edge to the liver pointed in the same direction, and the existence of an enlarged spleen was recognized as a further fact against this diagnosis, it being a familiar clinical experience that with malignant disease of the liver splenic enlargement is but rarely associated. Jaundice and ascites were both absent, a condition, however, quite consistent with a diagnosis of malignant hepatic disease, though each is a frequent result of such disease, at least in its later stages. The condition of the patient's general health, too, had manifestly an adverse bearing on the suggestion that he might be suffering from malignant disease.

As I have already pointed out, the "lump" projecting from the surface of the liver suggested an abscess, a hydatid cyst, or malignant disease, and we shall consider these proposals more in detail in the later part of the lecture.

The immediate position with which we had to deal was the apparent association of hepatic and splenic enlargement. Now a combination of enlargement of the liver with enlargement of the spleen may well be due to amyloid disease, which frequently affects both organs. The enlargement due to amyloid disease, too, is, like that in our patient, a painless one. But amyloid disease would not cause a localized lump on the surface of the liver; it affects all parts of the organ equally, producing a smooth and uniform enlargement. And when it is present it usually attacks not only the spleen and the liver, but also the kidneys and intestines, leading in the one case to polyuria and albuminuria, and in the other to diarrhœa. None of these symptoms were present in the case we are now considering, and therefore, amyloid disease seemed to be excluded from the diagnosis. This conclusion was strengthened, too, by the absence from the history of any event that would account for amyloid disease. The patient had never suffered from any long-continued suppuration (such as may occur in phthisis pulmonalis, or in tubercular disease of bone), nor from syphilis, and these are the recognized causes of amyloid disease.

Syphilis had to be still further considered, as this disease may enlarge the liver by causing gummatous deposits in the organ, or by leading to increase of the capsular and interlobular connective tissue. There was, however, no evidence of syphilis in any part of the patient's body, and his general bearing inclined us to receive his statement that he had never contracted the disease. For the same reason we believed his word that he had never been in the habit of taking alcohol to excess. And this was a matter of some importance, for alcohol in excess, as you know, may produce cirrhosis of the liver, and this, by obstructing the portal vein, may cause considerable splenic enlargement. No doubt in the majority of cases cirrhosis leads to diminution in the size of the liver; but this is not always so, and the liver may indeed be very definitely enlarged. Other features of the case, however, rendered it unnecessary for us further to consider the possibility of either syphilis or alcohol as a cause of the pathological conditions within the abdomen. There were, indeed, besides these conditions, other evidences of disease, namely, in the right chest, and manifestly the conditions above and below the diaphragm had to be regarded as possibly related to one another.

I remind you of the physical facts elicited in the examination of the right chest. Percussion of the right front detected dulness from the lower border of the third rib downwards, so that we had a continuous area of dulness from the third rib to the lower border of the hepatic dulness, this area measuring in the nipple line seven and in the middle line seven and a half inches. There was, too, deficiency in the movements of the right chest; the right side measured one inch in circumference more than the left; and over the dull area the respiratory murmur and the vocal fremitus and resonance were diminished. These physical signs mean, of course, fluid present in the right pleura, but it was observed that while in front, the evidences of abundant fluid were quite decided, clear percussion behind extended down to the ninth dorsal spine, that is, the dulness and the deficiency of the respiratory murmur were limited to the extreme base. Evidently, therefore, if fluid was present within the pleura, it was not free to find its level, but was, by some means or other, restricted in its movement and confined for the most part to the anterior portion of the pleural cavity.

Now, obviously, it was necessary to regard the thoracic condition in the light of the disease which undoubtedly existed below the diaphragm. It was, of course, possible to suggest that an accumulation of fluid within the right pleura had depressed the liver, and so had given to this organ a fictitious appearance of

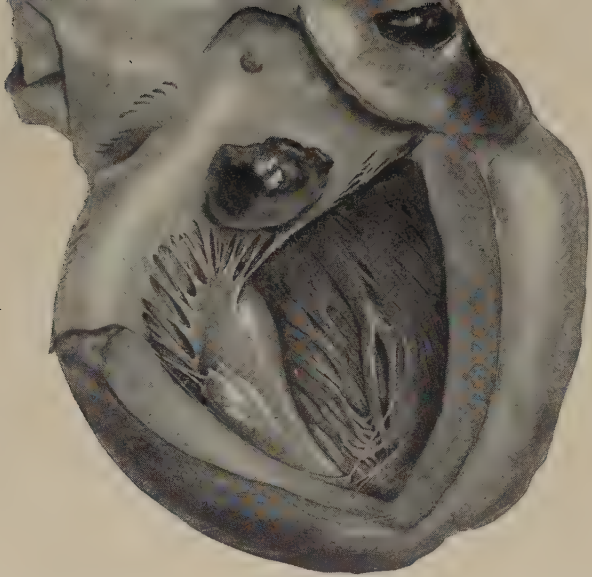


Fig. 39.—Infective endocarditis showing a large mass of fibrin adherent to one of the mitral curtains and a small area of ulceration on the wall of the auricle at some distance from the mitral orifice. No cardiac murmur was present during life.

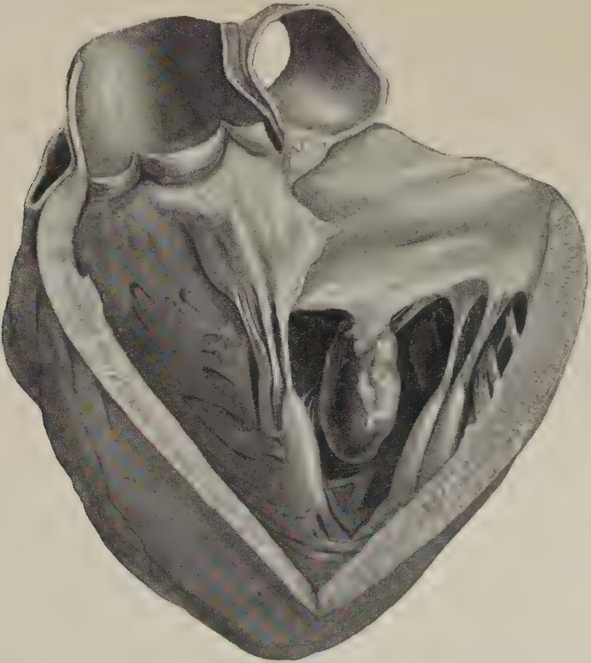


Fig. 40.—Infective endocarditis showing a large thrombus dependent from one of the mitral curtains into the ventricle. There was no cardiac murmur.

enlargement. The facts generally did not, however, support this view, and moreover, such a suggestion would not explain the presence of the tumour which we regarded as an enlarged spleen, nor would it account for the "lump" which appeared to project from the surface of the liver. It seemed, therefore, much more probable that the primary disease originated not above but below the diaphragm, and that the abnormal condition in the right chest was a secondary result. Hence we were thrown back once more upon the necessity of endeavouring to determine the nature of the disease responsible for the abnormal physical signs which we found to exist in the upper abdomen.

Now, as we have already seen, some of these signs, and more especially the enlargement of the liver, together with the presence of a "lump" or nodule on its surface, carried with them the suggestion that the case was one of malignant disease of the liver. On the other hand, the facts already considered, and especially the absence of pain and tenderness, opposed this diagnosis, or at least did not support it; and as our notes of the case accumulated we found that the temperatures, though pursuing an irregular course, were often decidedly febrile, which is not usual in cases of malignant disease. A high temperature is, however, possible with malignant disease, and too much weight, therefore, we felt ought not to be attached to the existence of the pyrexia.

Another suggestion, which to some extent arose from the presence of fever, was to the effect that we had to deal with pus in the upper abdomen, and that this was present either as an abscess in the liver or as a sub-diaphragmatic abscess. With one or the other it could be conceived that a collection of pus might explain not only the abdominal conditions, but also the physical signs in the right chest. Thus it might, by causing irritation of the right pleura, lead to the presence of fluid in the pleural cavity; or, by accumulating below the diaphragm, it might raise the vault of the diaphragm and so might interfere with the action of the right lung and cause the impaired percussion and deficiency of respiratory murmur detected over the front of the right chest. On the whole, too, it seemed more likely that these effects would follow such a collection of pus as is now suggested, than that a cancerous mass would be so localized in the upper part of the liver as to push up the diaphragm and cause extensive dull percussion over the right lower thorax. Yet there were difficulties in this diagnosis also. An abscess of the liver could scarcely produce the firm nodule we detected on the surface of the liver, nor was it easy to understand how it could lead to decided splenic enlargement. And though the patient, many years ago, had been resident for a time, first in Egypt and then in Buenos Ayres, he had never visited the tropical countries, such as India and China, where almost all the cases of large or tropical abscess take origin. Further, he had now resided at home for fully six years, and had enjoyed good health until his present symptoms developed, so that hepatic abscess seemed an altogether unlikely event.

The existence of a sub-diaphragmatic abscess, we recognized, might quite well have accounted for at least some of the abnormal conditions detected in the upper abdomen, and such an abscess, by pressing the diaphragm upwards, might, in addition, have explained the dulness in the right chest. The clinical evidences of a sub-diaphragmatic abscess are often very obscure. They depend, to some extent, upon the exact situation of the pus, and upon the degree of thickening and the adhesions produced in the neighbourhood of the abscess; pleurisy at one or both bases is a not uncommon complication. Such an abscess is usually secondary to some other condition, as for example a gastric ulcer, gall-stone, hydatid cyst, appendicitis, etc. But no conclusive evidence of any of these conditions existed here. There had been no severe pain after food, and no hæmatemesis, such as we frequently, but not invariably, find in gastric ulcer. Nor had there been the severe paroxysmal attacks of pain which indicate the passage of a gall-stone through the cystic and bile ducts; and jaundice had never been present. A hydatid cyst of the liver is not a frequent occurrence in this country, and there did not appear to be anything in the patient's history or symptoms to support the view that the case was one of this nature, except perhaps, it might be said that the splenic enlargement favoured hydatid disease of the liver rather than cancer.

The clinical position, therefore, was this: That there was some definitely abnormal condition in the upper abdomen, and also in the physical examination of the right chest, was certain, and it seemed probable that the thoracic condition was secondary to the abdominal. The nature of the disease in the upper abdomen was a matter of great difficulty. In spite of the high temperatures, the apparent enlargement of the spleen, the absence of hepatic tenderness, and other facts already alluded to, the presence of a definite nodule on the surface of the liver made it impossible to exclude absolutely the diagnosis of malignant disease of the liver. On the other hand, the possibility of a collection of pus situated below the diaphragm had to be recognized, though nothing like a confident conclusion on this point seemed possible.

In these circumstances, and recognizing that in the case of an abscess surgical measures might be of avail, it was decided to make an exploratory puncture. After a negative result in the right lateral region of the chest, the needle of the aspirator was introduced in the ninth intercostal space four inches from the spine. At first there was no result, the pleural cavity, or at least this part of it, appearing to be free from fluid. But on pushing home the needle so as presumably to penetrate the diaphragm, a considerable collection of fluid was tapped. This fluid was about the consistence of cream and of a light chocolate colour. Its reaction was neutral; its specific gravity 1050. Under the microscope it was seen to be crowded with large, irregularly-shaped cells, nucleated and highly granular. The quantity removed was 58 ounces. After removal of the fluid it was found that the right lobe of the liver, with its projecting nodule, had passed upwards behind the lower ribs, so as to be entirely beyond the reach of palpation, while the physical signs in the front of the right chest, as well as the condition of the splenic region, remained without substantial change. It was concluded, therefore, that the fluid had been removed from between the upper surface of the liver and the diaphragm, and that its removal had been followed by an ascent of the liver, the descent of the diaphragm presumably being prevented by adhesions. There now arose the question of the nature of the fluid. It was certainly not pus, nor was it the watery fluid to be expected on the puncture of a hydatid cyst. Was it possible that its source was a cancerous tumour? No doubt it is common to find that cancerous nodules, as met with in the liver, tend to soften in their central portions, so that, at least occasionally, they may present, even when examined through the abdominal parietes, a sense of obscure fluctuation; while, after death, such nodules on section may exhibit the escape of a creamy juice containing cancer cells. But it was difficult to picture the state of matters which would allow so large a quantity as nearly three pints of fluid to be produced in connection with a cancerous tumour of the liver. Still, there were the facts. The puncture was made when our minds were hesitating between a diagnosis of cancer on the one hand, and abscess on the other. Now the fluid certainly was not pus, and the cells might well be cancer cells, though none of our pathological staff would venture a positive opinion on this point. To gain every chance for the patient I requested one of my surgical colleagues to examine him, but in the end no operation was attempted and the patient died three days after the date of the paracentesis.

Fortunately, we were able to secure a post mortem examination, and from it we gain a satisfactory explanation of the case. Here, in the right lobe of the liver, we find a most remarkable cystic development (fig. 41) occupying almost the whole of the right lobe. It includes several cysts, the largest having a diameter of about 4½ inches. The inner wall of each cyst has an exceedingly irregular surface, and it is from these cysts that the fluid was withdrawn through the aspirating needle. There are, also, a few cancer nodules scattered over the surface of the liver, and the most prominent of these, situated on the convex surface near the anterior border, is the nodule which attracted our attention in the clinical examination. The diaphragm is adherent to the base of the right lung, and the pleural cavity is free from fluid.

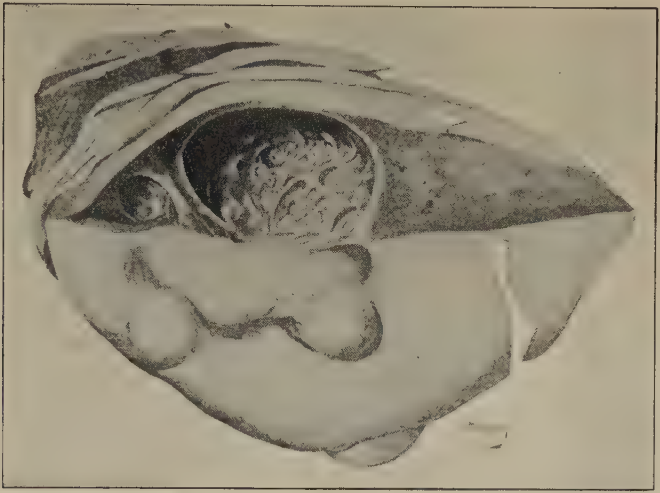


FIG. 41.—Liver showing cysts cut open by a vertical incision passing from right to left. From a drawing by Dr. Alex. Macphail.

There are two features of this cancerous growth in the liver which are unusual. The first is the collection of the cancer mass mainly towards the upper part of the organ, so that the liver was enlarged by it upwards in the direction of the chest rather than downwards in the direction of the abdomen. The second unusual feature is the extreme cystic condition. It would appear that a number of cancer nodules had become grouped close together in the upper part of the right lobe of the liver, and that the process of central softening, common in cancerous nodules, had been carried to an extreme degree, so that large collections of fluid were formed. Thus it happened that a cancerous formation in the liver produced a collection of fluid which bulged upwards and compressed the base of the right lung, an event well within recognition as a possible result of abscess or of hydatid cyst, but certainly quite unusual in cancer.

The cancer of the liver is, as is usual, a secondary growth, and the primary tumour, as is very frequently the case, exists in the stomach. It has the form of a nearly circular ulcer, 2 in. in diameter, with indurated edges, and showing in its floor an irregular perforation which passes through the entire thickness of the stomach wall. This ulcer is on the posterior wall of the stomach 5 inches from the pylorus, and its inferior margin just reaches the great curvature of the stomach. The perforation does not lead into the peritoneal cavity, but into a tumour mass which passes upwards and to the left and is adherent to the lower and inner surfaces of the spleen. This tumour manifests the cystic tendency exhibited in such an extreme degree in the growth in the liver, and it, as well as the other formations, has the structure of a cylinder-celled epithelioma. It is noteworthy how this tumour mass, connected with the great curvature of the stomach and the spleen, simulated an enlarged spleen, and in this way established a presumption against the diagnosis of malignant disease of the liver.

There is one other feature of the case which may be alluded to in a word. It is the contrast between the symptoms from which the patient suffered, and the physical evidences of disease detected on examination. Here is a man who, it is true, had complained for several months of loss of appetite and of some pain after food, but who had not the slightest suspicion that he was seriously ill. Yet, during this time, a malignant ulcer had penetrated through the wall of the stomach, and the malignant process had produced both a considerable tumour in the left hypochondrium and extensive secondary developments in the liver. The contrast is thus a very forcible illustration of the danger of falling into very grave error if you neglect to make a thorough physical examination, and this even in cases where the patient's complaint does not suggest any serious disease. Pain in the stomach and other evidences of dyspepsia may be comparatively slight, and vomiting and hæmatemesis entirely absent, while, as is here seen, a considerable cancerous growth is taking place in the stomach wall. The evidences of gastric cancer, in short, may, at least for some time, be restricted to some slight dyspeptic disturbance with more or less decline of flesh and strength. Such a combination of symptoms coming on in middle life must always be subject to suspicion, and must claim careful and repeated physical examination. This is one of the practical clinical lessons to be learned from the case which has occupied our attention to-day. The facts of the case, as we observed them from day to day, you already have in your note-books. It will be well for you carefully to compare them with the results of the post mortem examination. If you do this, the case will, I think, remain with you as a clinical lesson of permanent value.

A CLINICAL LECTURE ON A CASE OF PYO-PNEUMOTHORAX DUE TO GANGRENE OF THE LUNG.

GENTLEMEN,—The case of the man R. K—, aged 56, who died three days after his admission to the hospital, is one well worthy of careful study. I propose to-day to direct your attention to the facts of the case, and to the lessons which they teach us.

Let me remind you of the observations which we made together at the man's bedside. We saw him for the first time very shortly after his admission, and it was at once evident that he was seriously ill. Not only was the pulse unduly rapid—it numbered 120—but there was also great increase in the frequency of the respiratory movements. These reached nearly 60 per minute, and were further increased by effort, as in any attempt to answer questions. Hence we decided not to disturb the patient more than was absolutely necessary. With all this distress, however, there was no orthopnœa, and no lividity of the face or other parts; nor did the patient complain of shortness of breath, but only of some pain or discomfort in his right chest; and the temperature registered only 98°.

Apart from the general prostration of the patient, the most anxious and conspicuous symptom was the very rapid respiratory rate. Now it was certain that this quickening of the respiration was not due to fever, for the temperature was practically normal. Further, while a high temperature *per se* will quicken the respiration, it will at the same time quicken the heart's action to a corresponding degree, so that the normal pulse-respiration ratio of 4 or 4.5 to 1 remains undisturbed. But here the respiration was quickened out of proportion to the pulse-rate—the pulse-respiration ratio, indeed, was practically as 2 to 1. Such disproportionate acceleration of the respiration always throws suspicion upon the respiratory apparatus, and we proceeded therefore at once to examine the chest. There we readily detected evidences of serious disease. Movement of the right chest was distinctly deficient; there was neither vocal fremitus nor vocal resonance; over the whole right front percussion was tympanitic and the respiratory murmur scarcely to be heard; the right back gave dull percussion and absence of the respiratory murmur over the base, with a harsh bronchial respiratory murmur in the interscapular region. Further, with the patient sitting up, it was observed that the lower part of the right front, which had previously been tympanitic, had become dull; and on gently shaking the patient (Hippocratic succussion) we heard a distinctive splashing sound. This sound inevitably means the presence of both air and fluid in a space or cavity, and heard, as in the present case, over the right chest, it was open only to one interpretation, namely, the presence of air and fluid in the right pleural cavity. Under the pressure of these the lung would be, as it were, crushed into the upper and back part of the chest, so that while the breath sounds were absent from the greater part of the chest, they were of a harsh, bronchial character over the condensed lung in the interscapular region. All the other physical facts supported this conclusion. The deficient movement and the absence of breath sounds showed non-expansion of the lung during inspiration; the absence of vocal fremitus and vocal resonance meant separation of the lung tissue from the chest wall; while the percussion, tympanitic in one part and dull in another, the boundaries of the two areas varying with the position of the patient, was exactly what, on physical grounds, is to be expected when the pleural cavity contains a quantity of air and fluid free to move under the influence of gravitation. To all these was added the final proof afforded by the splashing sound. As you will remember, I put it to you at the first examination at the man's bedside, there was not the slightest opportunity for doubting that the case was one of air and fluid (serous, purulent or hæmorrhagic) in the cavity of the right thorax.

The only noteworthy physical signs, other than those above recorded, were the presence of the cardiac impulse in the vertical line of the nipple (in the fifth space), and of the left border of cardiac dulness somewhat to the left of the nipple. The interpretation of course is, that the increased pressure in the right pleural cavity had displaced the heart somewhat to the left. It is when such increased pressure exists in the *left* pleural cavity that the most conspicuous evidences of cardiac displacement are found; then the heart may be so far displaced to the right—dexiocardia—as to present the impulse in the neighbourhood of the right nipple. But fluid or air, also, in the right pleura may, as in the present case, displace the heart, though as is to be expected less conspicuously than when the pressure is applied on the left side. Fluid or air in the right pleura may, too, displace the liver downwards, though we could not detect evidence of this in our present patient.

Our first examination, then, brought us to this point, that we had to deal with a case in which both air and fluid were present in the sac of the right pleura. The nature of the fluid was made certain on the following day, when, in consequence of increasing dyspnœa, we tapped the right chest and drew off 50 ounces of a highly offensive purulent fluid, and this on examination showed numerous streptococci and staphylococci, but neither pneumococci nor the tubercle bacillus. It was in this way made certain that the case was one of right pyo-pneumothorax.

This, however, was far from a complete diagnosis, for pyo-pneumothorax is never a primary event. We had, therefore, to try to determine the cause of the pyo-pneumothorax in this individual case. This, in short, was the diagnostic problem which the case presented. Now, it may be taken as practically certain, that when air is present in the pleural cavity, it has either been admitted through a wound of the chest-wall, or has entered from some air-filled viscus which has ruptured into the pleural cavity. At one time it was believed that under certain conditions the pleura might " secrete " air, but such a suggestion will hardly be advanced to-day. It is also stated that pus or other fluid in the pleural cavity, may, by decomposition, give rise to a collection of gas within the pleura and may thus produce a condition of pyo-pneumothorax. If such an event ever does occur, it is certainly most exceptional. The rule that I have just stated must remain without serious qualification, and therefore, given, as in the present instance, an absence of any external wound, we were compelled to consider in what manner air from some air-filled viscus had managed to enter the pleural cavity.

Naturally, in such circumstances, suspicion first falls on the lung. Occasional cases are recorded in which cancer or some other ulcerative lesion has led to the entrance of air into the pleura from the œsophagus, stomach, or intestine; and of course, air so admitted may carry with it septic irritants which may induce purulent inflammation. We certainly had no evidence that the present case was one of these clinical curiosities. The probability in this, and indeed in every case of pyo-pneumothorax, is that the air has escaped from the respiratory tract. If our patient could have been shown to have had a fractured rib, we might have held that the pulmonary pleura had been torn by the broken bone, and that in this way air had escaped from the lung. But there was no evidence of such a fracture, and indeed no history of violence. The man, according to the statement of his landlady, had been in his usual health until nine days before admission; he then gave up his work on account of " weakness," which was followed by a good deal of vomiting; shortly afterwards he complained of " a severe cutting pain " in his right side, and was troubled much by cough, accompanied by a fairly copious white frothy spit; and it was not until three days before he came to the hospital that he had consented to remain in bed. He had been a hard drinker for many years.

How far did this history help us to determine the cause of the pyo-pneumothorax? Well, it certainly rendered it extremely unlikely that the case was an example of what occasionally happens, namely, that a collection of pus opens from the pleural cavity into the lung. An empyema, or even an abscess of the liver, may discharge itself in this manner, and may thus allow air to enter the pleural cavity. But neither of these conditions will develop, and proceed as above described, in the course of a few days. The history of the case and the generally well-nourished condition of the patient were opposed to any such proposal, and we had no account of any sudden copious expectoration of pus such as is likely to occur when a purulent collection from outside the lung suddenly communicates with the respiratory tract; and the sputum, as we saw it, was merely frothy mucus. We were therefore compelled to believe, that, as is the usual experience, there had been in the first place some lesion of the lung substance; that this had led to perforation of the visceral pleura; and that the perforation had been followed by the entrance of air and septic matter into the pleural cavity, with the consequent production of a purulent pleurisy.

Now the lesion which most commonly causes such a perforation of the pleura, is a tuberculous deposit near the surface of the lung. This leads to sloughing of a small patch of the visceral pleura, and to the consequent escape of air from the respiratory passages into the pleural cavity. Could such an explanation apply in the present case? Certainly there was nothing in the patient's general appearance to suggest tubercle. On the contrary, he was an unusually strong-looking, well-nourished man; and the left lung was normal to physical examination. These facts, however, did not exclude tubercle from our consideration, for the most pronounced cases of pneumothorax occur when the tuberculous changes in the lung are but little advanced. In the more chronic cases of phthisis pulmonalis, the pleura becomes thickened, and therefore not readily perforated, as a result of chronic inflammation; in consequence, too, of the formation of adhesions, even if pneumothorax does occur, it is apt to be limited, and not, as it were, to involve the whole pleural cavity. In such circumstances a pneumothorax may cause very little disturbance. But given, on the other hand, an early deposit of tubercle so situated as to lead to perforation of a previously healthy pleura, then, when perforation occurs, there will be a sudden rush of air into the whole pleural cavity, and consequently, a sudden compression of a lung the greater part of which until that moment had been physiologically efficient. It is under these conditions that the acute manifestations of pneumothorax reach their most extreme form. Indeed, you may take is as a sound clinical rule, that any acute pneumothorax for which no explanation can be found, justifies a definite suspicion of pulmonary tubercle; in exceptional instances time fails to confirm this suspicion, which, however, always remains until contradicted by the course of events.

We could not, therefore, on the ground that our patient showed no evidence of tubercle in his left lung, and because his general nutrition was good, positively exclude tubercle from the diagnosis; and the failure to find the tubercle bacillus, while of value up to a certain point, by no means closed the discussion. We had, too, the additional statement that three of the patient's children had died of "decline," a record which might well have been urged to strengthen the suspicion of tubercle necessarily induced by the occurrence of pneumothorax without manifest explanation.

There was, however, in the case one feature which led us seriously to question the diagnosis of tubercle, and this was the rapidity with which (presuming the history could be relied on) the fluid had become purulent. This, and the extremely offensive character of the fluid, seemed to show the agency of some much more virulently septic influence than the tubercle bacillus. No doubt, if a patient recovers from the acute disturbance produced by a pneumothorax occurring in an early stage of phthisis, fluid will begin to accumulate in the chest, and may, indeed, entirely displace the air and completely fill the pleural cavity; and the fluid may certainly become purulent. But that in the course of a few days a large quantity of stinking pus should be found as a result of a pneumothorax dependent on pulmonary tubercle, seemed hard to believe. It was mainly this consideration which led me to suggest that some more distinctly septic irritant than tubercle was present, and that possibly the lesion in the lung was of the nature of gangrene.

In the clinical sense of the term, gangrene of the lung, unless used with some qualification, implies a disease widely spread through both lungs and characterized by an extremely fœtid and peculiar smell of the breath, and by abundant and highly offensive sputa. These features certainly were not present in this patient, and no one would have suggested a diagnosis of *diffuse* gangrene of the lung. But it is well known that the gangrenous process may be *circumscribed,* and that a limited patch at the surface of the lung is an occasional cause of pneumothorax. And in reference to the absence of a foul odour in the breath and sputa, we must remember that the affected lung was practically out of action, being in a state of collapse, and, as we found on examining the chest after paracentesis, it showed no evidence of re-expansion on the removal of the fluid. Some support to the diagnosis of gangrene was afforded by the history of the patient's habit of intemperance, for, whatever be the immediate influence which sets up the necrotic process, chronic alcoholism is well recognized as a predisposing cause of pulmonary gangrene.

Our clinical observations, then, had brought us to this position: We were sure that the case was one of right pyo-pneumothorax; we had every reason to believe that the pyo-pneumothorax was secondary to some lesion of the lung; the highly septic character of the pus, taken together with the history of only a brief illness, made us doubt whether tubercle could be the responsible pulmonary lesion; and in these circumstances we concluded that probably the immediate cause of the pyo-pneumothorax, and the first event in the clinical evolution of the case, was a limited patch of gangrene at the pleural surface of the lung.

The post mortem examination showed the suggestion of gangrene to be correct. At the upper part of the fissure between the two lobes, there was, on each opposing surface, a patch of necrosed pleura; below this was an area of gangrene, with a superficial extent of some two inches square and extending into the lung to the depth of about an inch. Here was the explanation of the pyo-pneumothorax. The only other lesion of interest was one which, if we had been able to detect it, might have inclined us more strongly to a diagnosis of tubercle. In the apex of the left lung were some pigmented cicatrices and cretaceous nodules, doubtless the result and expression of a healed tuberculous lesion. Such appearances are very common, and they indicate the method by which, in favourable circumstances, tubercular processes are brought to a satisfactory conclusion.

In the treatment of the case we could not expect much encouragement. But a review of it may be of some value as helping to guide you in dealing with any case of pneumothorax causing acute symptoms. The patient, of course, was kept absolutely at rest, and we gave him whatever nourishment he could take, which was not much; to relieve the pain in his side poultices were used, and when pain was very severe a small dose of morphine was injected hypodermically. With the same object, one or two leeches might quite well have been applied. Stimulants were demanded by the poor quality of the pulse rather than by its rapidity, and as dyspnœa was severe and increasing, the chest was tapped. The same principles would guide you in dealing with any case of pneumothorax seen immediately on its occurrence, and this you must remember is one of the emergencies of practice. The only caution I ought to add, is, that if you decide to remove some of the air on account of the urgency of the dyspnœa, you should do this with a hollow needle and not with an aspirator, allowing air to escape under its own pressure, and not using suction to extract it. If you use an aspirator you may reduce the pressure of the air in the pleural cavity below the pressure of air in the lung; and this may be followed by an opening up of the wound in the visceral pleura, and by a fresh rush of air into the pleural cavity.

There is one other question I have been asked, namely, "Why did this man not have a high temperature?" The answer is, "I do not know." No doubt for a time after the "shock" of a pneumothorax the patient is apt to have a subnormal or collapse temperature, but you do not expect this to continue for several days. And if one circumstance more than another might be expected to produce febrile disturbance, surely it is the presence of highly septic pus in a serous cavity. But the present instance shows you that this expectation is not always realized; and a similar experience is not infrequent in patients suffering from septic peritonitis. It may, perhaps, be suggested that the inflamed membrane prevents absorption, but our patient's general prostration and the presence of muttering delirium, etc., were doubtless the result of septic absorption, which, indeed, I presume, was the immediate cause of his death. Hence the absence of absorption through the inflamed pleura can hardly be adopted as an explanation of the low temperature record. Someone suggested that the man was so prostrated, and his vitality so low, that he could not "produce" a high temperature. But neither will this fit the facts, for while during the greater part of the three days he was here no record exceeded 99° F., the temperature some six hours before his death suddenly ran up to 101.8° and remained about that level until the end. If we cannot find an explanation, we can at least take a note of the fact. It is one of the several features of the case well worth adding to the store of our clinical experiences.

A CLINICAL LECTURE ON A CASE OF ALBUMINURIA CARDIAC HYPERTROPHY AND RETINITIS.

GENTLEMEN,—This patient, a man of 50 years, is reported to have been suffering, during the past year or so, from anæmia. He has on several occasions, we are told, improved for a time, but pallor and breathlessness have returned again and again, and at times a small quantity of albumin has been detected in his urine. I need hardly say that anæmia, without qualification, is never a quite satisfactory diagnosis, more especially in a male patient who has reached middle age. In young women, it is true, we often have to be content to label cases simple anæmia or chlorosis because such a condition exists as a matter of actual clinical experience. And experience, here, provides this additional excuse, that although we are unable to explain the disease, we know that as a rule we can cure the patient. Yet, even in the case of a young woman, it is hardly wise to be content merely to recognize the anæmia, to label the case chlorosis, and to order the administration of iron. In other words, every case of anæmia in a young woman cannot safely be regarded as an example of chlorosis. Apart from the possible presence of some organic disease, phthisis pulmonalis for example, the anæmia may depend on such factors as insufficient or unsuitable food, carious teeth, imperfect digestion, constipation, loss of blood, defective hygienic surroundings, etc., etc. If any of these exists, iron will at best attain but a partial success. Consequently, even in young women, the recognition of anæmia claims at least an effort to determine its causation. And in adults, and more especially in male adults, anæmia ought never to be a resting place either for diagnosis or for treatment. It is a symptom arising in many and varied circumstances, and some of these are of serious import. Therefore, as it is at once evident that our present patient is anæmic, we must proceed to make a physical examination to try to discover on what, in this particular case, the anæmia depends. If we fail to detect any organic lesion, we may have to proceed to make a microscopical examination of the blood; and if this, and all other methods, fail to lead us to a diagnosis, we shall still be bound to suspect the existence of some serious mischief which has not yet come within the reach of physical examination.

Now when I uncover the patient's chest, it is manifest that he has a considerable degree of cardiac hypertrophy. The impulse extends to the sixth space, and is an inch or more outside the nipple line. In the epigastric notch, too, there is a visible and tangible pulsation as if from enlargement of the right ventricle. Further, over the apex region I hear a very distinct systolic murmur, and this is heard also, and with about the same degree of intensity, along the line leading from the cardiac apex to the foot of the sternum. The murmur does not extend towards the left axillary line, as is the fashion in murmurs of mitral origin; and the fact that the second sound is much louder over the aortic than over the pulmonic area, is also opposed to the suggestion of mitral disease. From its area of distribution the murmur suggests a tricuspid rather than a mitral source, and it possesses the "superficial" whiffing character which tricuspid regurgitant murmurs usually possess. Of course, tricuspid regurgitation cannot account for the enlargement of the left ventricle, while a condition of mitral regurgitation would be a possible explanation of the existing extension of both sides of the heart. There may, however, be in the case some other factor which will help us to get rid of any uncertainty as to the physical cause of the systolic murmur. It is hardly necessary to say that we cannot regard the murmur as functional or hæmic. Its presence over the ventricular area and not over the base, and the fact of cardiac hypertrophy, are sufficient to negative such a suggestion. And neither the hypothesis of mitral disease, nor the supposition of tricuspid disease, throws any light on the cause of the anæmia. There is, it is true, one form of cardiac valvular disease which is often associated with an anæmic appearance of the patient, namely, aortic regurgitation. But mitral or tricuspid regurgitation, if they affect the colour of the patient at all, do so in the direction of cyanosis rather than of anæmia. Possibly, when an abnormal condition of either of these valves is the result of a recent attack of rheumatic fever, the anæmia produced by this disease may still be present. But it is then a consequence of the rheumatism, not of the valvular flaw. In the present instance we cannot explain the anæmia in this fashion, because the patient, according to his own statement, has not had rheumatic fever either recently or indeed at any time. Therefore, while we have no doubt that there are organic changes in the heart, we are still without an explanation of our patient's anæmia.

Let us now examine the urine. We can only obtain a small sample, but it is quite sufficient to show the presence of albumin in moderate amount. But, so far, this only raises another problem. For the albuminuria may mean either primary kidney disease (which might account for the anæmia), or it may be an expression of passive congestion of the kidney due to the cardiac-valve defect. Certain facts in the case seem at first sight to support the latter explanation. Thus, there is, as you see, a moderate degree of œdema in the legs and feet, and on examining the lungs you find impairment of percussion over the bases and some muco-crepitant râles. There is, therefore, some measure of passive engorgement both in the pulmonary and systemic circulations, and the latter may well include passive congestion of the kidney with resulting albuminuria. Undoubtedly, therefore, there are signs of cardiac failure, and the albuminuria may possibly be part and parcel of this failure.

Is the present case then one in which cardiac disease was the primary fact, and the renal condition a secondary development; or, on the other hand, is it one in which a primary disease of the kidneys has led to cardiac hypertrophy and dilatation? A combination of the physical evidences of heart disease with albuminuria frequently leads to a discussion of these alternative possibilities, and sometimes the question is one not easy to answer. In the present instance, fortunately, we can decide without much doubt that the primary event was almost certainly the renal disease. And this for several reasons.

First, in the urine itself we have evidences which suggest that the quantity is not diminished; and diminution is to be expected in passive congestion of the kidneys. The colour, as you see, is very light, almost watery in fact, and the specific gravity is low. The patient believes that he passes urine in excess, and he certainly passes it too frequently, as for many months he has been compelled to rise two or three times every night to empty his bladder—a symptom which, occurring in middle age, or even before this, is strongly suggestive, in the absence of disease of the bladder and of diabetes, of chronic kidney disease. Hence, from the history of the case, and from the physical characters of the urine, we are justified in concluding that the urine is passed, not in diminished, but in excessive quantity. Therefore the albuminuria cannot be explained as due to passive congestion of the kidney existing as a consequence of cardiac valve disease.

Again, the state of the heart itself is not one which suggests a primary endocarditis. The systolic murmur appears to depend upon regurgitation at the tricuspid orifice, and this, as we know from experience, is very rarely the result of endocarditis. It means, rather, that the right ventricle and its auriculo-ventricular orifice are dilated, so that the curtains of the valve fail to close the orifice during systole. This dilatation is always secondary to circumstances which strain or exhaust the energy of the right ventricle, as for example, mitral disease, pulmonary emphysema, or long-continued malnutrition. Further, our patient has considerable hypertrophy of the left ventricle, which tricuspid regurgitation will certainly not explain. Therefore, the state of the heart, not less than the condition of the urine, opposes the view that the case is primarily one of cardiac disease with passive congestion of the kidney and albuminuria as secondary consequences.

On the other hand, there is much in the examination to show that the original evil was kidney disease, and that the cardiac and circulatory conditions are the results of this. First, hypertrophy of the left ventricle, which is a prominent feature in the case, is a frequent associate or consequence of chronic interstitial nephritis, and this disease will account also for the exaggerated aortic sound, for the arterial degeneration which is present, for the albuminuria, and for the excessive secretion of urine. In time, cardiac hypertrophy is succeeded by dilatation, and if we conceive that in the present case dilatation has occurred and has extended to the right ventricle, we have an explanation of the systolic murmur, of the congestion of the bases of the lungs, and of the œdema of the lower limbs. There therefore seem to be substantial reasons for regarding the case as one of chronic interstitial nephritis with commencing cardiac failure.

If any doubt as to the accuracy of this diagnosis should still remain, it is at once dispelled by the use of the ophthalmoscope. There is in each eye an appreciable degree of optic neuritis, and, in lines radiating from the macula, are numerous white spots and patches such as are often described, though not quite accurately, as distinctive of albuminuric retinitis. This condition is certainly never produced by passive congestion of the kidney. Taken together with the other facts of the case, it concludes the diagnosis that in the present patient we are dealing with a case of kidney disease. And the age of the patient, the insidious onset, the absence of œdema until cardiac failure has developed, the considerable hypertrophy of the left ventricle, the arterial degeneration, and the abundant urine with albumin in relatively small amount, render it practically certain that the form of kidney disease present is chronic interstitial nephritis.

In reference to prognosis, our patient presents rather serious facts. The chance of curing chronic interstitial nephritis is, in any event, a very slight one, though with care the patient may often live for many years. But here we have signs of serious import. In the first place, there is retinitis. This may be associated with albuminuria occurring in the course of pregnancy without necessarily increasing the gravity of the prognosis. It may even occasionally be found in renal disease apart from pregnancy, and yet the patient may recover. But this result is rare, and is always, or nearly always, I believe, found in cases where the renal disease is acute or subacute and is of the parenchymatous form, with œdema as an early and prominent symptom. Certainly in chronic interstitial nephritis, the development of retinitis is usually a sign of grave omen. In addition, we have, also, decided evidences of cardiac failure, namely, œdema of the lower limbs and of the bases of the lungs, and a pulse which, though of moderately high tension, is not quite regular. Further, the state of the respiration is, I think, a claim for even more urgent alarm. Everyone must have observed the respiratory distress exhibited by the patient—a degree of distress quite out of proportion to the physical facts detected on examination of the chest. This, however interpreted, is a very anxious or even an alarming fact, and it may well be the commencement of severe uræmic manifestations. Altogether the condition of the patient, and especially the embarrassed respiration, gives evidence of serious danger to life even in the immediate future. Distressed or rapid breathing is always a serious condition, and this especially in an adult; even with what appears to be a satisfactory state of the pulse, respiratory distress has invariably an anxious prognostic significance.

The first measure of treatment for our present patient is to keep him in bed and to restrict his diet to milk. The bowels should be freely opened, say by compound jalap powder. By saline diuretics, as acetate or citrate of potassium, the kidneys may be rendered more active, and some sweet spirit of nitre will help to secure diaphoresis. A saturated solution of cream of tartar taken *ad libitum* is valuable, both as a diuretic and as a saline purgative. Pilocarpine, I think, would be risky in view of the evidences of œdema of the lungs, but a steam bath, while the patient is lying in bed, might be ordered. Unless the action of the heart promptly improved under rest I should add digitalis to these remedies, combining it with nitro-glycerine in order to neutralize its contracting effect on the smaller arteries. Guy's pill, containing digitalis, squill, and blue pill, is often an effective diuretic, and though it is usually said that mercury is contraindicated in albuminuria, I should certainly try it for a time, in the event of the other treatment failing. At the best, you can hardly expect to do more than prolong the patient's life for a few months, and there is at least a chance of a fatal termination at a much earlier date.

A CLINICAL LECTURE ON TWO CASES OF CHRONIC RENAL DISEASE ASSOCIATED WITH SECONDARY INFLAMMATIONS OF THE SEROUS MEMBRANES.

Gentlemen,—The cases on which this lecture is offered as a commentary are not to be classed among clinical experiences of a rare and occasional order. On the contrary, the disease from which each of the patients suffered is a very common one, and even the manner in which it here displayed itself is by no means very unusual. There are, however, in such cases, certain clinical features which are apt to cause serious error both in diagnosis and prognosis, and hence the present examples may perhaps be studied with advantage.

The first case is that of a man, aged 57, admitted to hospital with a complaint of severe pain in the upper right abdomen, or, as he put it himself, "over the liver." The pain came on suddenly seven days before admission, and it had apparently caused the patient great distress. Prior to this, his history was that of a strong, healthy, temperate man. His general appearance was in entire harmony with this statement, and in his own view he was in perfect health except for his present attack of pain.

Perhaps the first suggestion which occurs to the mind on hearing of the sudden development in a healthy adult of an acute pain in the region of the liver, is that of gall-stone. All speculation in this and other directions, was, in the present instance, rendered unnecessary by the detection over the extreme base of the right lung of undoubted evidences of pleural friction. At first sight, therefore, the announcement of pleurisy at the right base seemed both a confident and a sufficient diagnosis. The only feature of the case apparently calling for comment was the reference of the pain and tenderness, not to the site of the pleurisy, but to the upper abdomen. Such an occurrence is presumably explained by the course of the lower intercostal nerves, and in any event, the discovery that abdominal pain is due to disease within the thorax is not an uncommon clinical experience. Indeed, the first clinical lesson of the present case is to emphasize the call to remember that abdominal pain may find its interpretation in an abnormal condition existing, not below the diaphragm, but above it. Many illustrations of this truth have been recorded in the shape of mistaken diagnoses, and they all combine to enforce the oft-repeated claim for thoroughness and completeness in physical examination. This doctrine, too, receives further emphasis from other aspects of the case now under review. In the circumstances, it might have been concluded, and not altogether unreasonably, that a recognition of the existence of an acute pleurisy exhausted the diagnosis. An apparently healthy man in the prime of life is suddenly seized with acute pain, and physical examination detects pleural friction. Is there any necessity to attempt to go behind these facts? They form, it would seem, a definite and well-understood clinical picture. All the same, had the diagnosis halted at this point a very grave error would have been committed.

What then were the results of further examination? In the first place, it was observed that the temperature record was rather below than above the normal level—a somewhat startling fact in a case of acute pleurisy with severe and persisting pain.

Again, examination of the urine showed a moderate quantity of albumin, and the microscope revealed many hyaline and fatty tube casts. Hence it became necessary to consider whether the case possibly included the presence of organic renal disease. No doubt, in many acute febrile processes—such for example as the specific fevers, pneumonia, etc.—albumin may appear in the urine without implying any permanent disease of the kidneys, and without sensibly modifying the prognosis. It is generally assumed that such a condition depends on the febrile temperature, and on the disturbances of which this is an expression. But in the present case there was no febrile temperature, and this in itself was sufficient to give pause to the conclusion that the albuminuria was a mere consequence of the pleurisy, and that it might, therefore, be dismissed as of no serious moment. Further, while the so-called febrile albuminuria may be associated with the presence of tube casts, these are not usually numerous and granular as in the present case. Indeed, the existence of many casts showing fatty degeneration carries a strong suggestion of definite renal disease of a chronic order. And yet again, the urine was not sensibly diminished in quantity; it was pale in colour, and its specific gravity was less than 1010—that is, its physical qualities were quite unlike the concentrated urine that usually attends a febrile process. Thus the absence of fever, the general characters of the urine, and the detection of numerous fatty casts, were facts difficult to reconcile with the view that the albuminuria was a mere symptomatic expression of the general disturbance caused by an acute pleurisy.

Nor was this all. Examination of the præcordial region readily detected the signs of considerable hypertrophy of the left ventricle. The cardiac impulse was felt in the sixth intercostal space and fully two inches to the outer side of the vertical line of the nipple. The force of the impulse, and the vigour of the sounds, at once negatived the view that the hypertrophy could be due to an acute dilatation caused by the severe pain or other constitutional disturbance from which the patient was suffering. The condition was certainly, therefore, one of hypertrophy, and the absence of any murmur on auscultation, excluded the possibility that the cause of this hypertrophy could be disease of one or other of the cardiac valves. On the other hand, the presence of visible pulsation and undue rigidity in the radial and other arteries, the existence of a pulse of sustained tension, and the marked accentuation of the aortic second sound, showed the cardiac hypertrophy to be associated with a general condition of arterial degeneration and with abnormal tension in the arterial system. But, as is well known, these changes in the circulatory apparatus are among the generally recognized associations of one form of kidney disease, namely, chronic interstitial nephritis. Hence, the conclusion suggested by the examination of the urine was placed altogether beyond question by the results obtained on examination of the cardio-vascular system. The records obtained from these two sources rendered it inevitable that in the diagnosis of the case chronic interstitial nephritis must of necessity be included. Had it been possible to have examined the fundus oculi, still further confirmation might have been obtained in the shape of neuritis, retinitis, or vascular changes. But the patient was so restless, in consequence of the pain he was suffering, that the ophthalmoscope could not be successfully employed. Even without it, however, and in view of the above facts, the diagnosis of chronic interstitial nephritis was written in plain and unmistakable characters.

But this is a position far removed from that suggested by the clinical facts which first came under observation. These facts, considered alone, offered merely a diagnosis of acute pleurisy, and on the whole they accommodated themselves so satisfactorily to this diagnosis that there was some temptation to leave the case as an ordinary example of pleurisy affecting the right side. A more responsible and exhaustive collection of facts, however, by introducing conclusive evidence of the presence of organic renal disease, showed the necessity for a considerable advance from this position. Such a process avoided—and none other would have avoided—a grave error in diagnosis, and this, had it been committed, would have involved an equally serious mistake in prognosis. For, whatever anxieties may be attached to a case of so-called " simple " acute pleurisy, they are as nothing compared to those suggested by the development of an acute pleurisy in the course of a case of chronic nephritis. The pleurisy, in such circumstances, is in all probability an expression of defect in the excretory power of the kidney, and results from the retention of waste products in the blood; and the prognosis in all such conditions is a most anxious one. Thus it is written in conspicuous characters across the record of the present case, that to secure an accurate diagnosis and an informed prognosis, a complete examination of the patient is always essential, and this, even when a less extensive investigation seems to afford what may be regarded as a sufficient explanation of the symptoms.

Next appears the conclusion that a patient with chronic interstitial nephritis may reach an advanced stage of the disease without any symptoms causing him anxiety or even annoyance. The first active manifestation of this disease may be, as the present record shows, an acute pleurisy, and this, it is to be specially noted, may develop without any appreciable elevation of the temperature. Indeed, it is not too much to say that an acute pleurisy occurring in a patient who has reached middle life, is an event which ought, in itself, to excite a suspicion of kidney disease; and the suspicion is all the more keen if there is at the same time no thermometric evidence of fever. Exactly the same remark may be made of pericarditis, and here, more particularly, there may be not only absence of fever, but also absence of pain.

This latter proposition is illustrated by our second patient—also a man aged 57—admitted to hospital, shortly after the case above described, with a complaint of breathlessness and general weakness. Albuminuria and tube-casts were detected on examination of the urine, and during patient's residence he developed the physical signs of consolidation of the upper lobe of the right lung and blood-tinted sputa, exactly as these events appear in an acute pneumonia. About the same time, too, coarse pericardial friction was heard. Yet, in spite of these complications, the temperature never exceeded the normal, and the patient made no complaint of pain or other discomfort capable of being referred either to the pneumonia or to the pericarditis.

This "quiet" character of the secondary inflammations consequent on kidney disease, is an important feature in the natural history of such disease, and unless it is remembered the occurrence of these developments may be overlooked, and thus their prognostic significance may be lost. The appearance of such inflammations is, unfortunately, a sign of bad omen. They are, only too frequently, terminal events in the history of renal disease, and to mistake them for "simple" inflammations, and to speak of their probable course in terms based on such a view, is more than likely to receive a disastrous correction. The cases here reviewed illustrate this proposition, for the first patient died on the seventh, and the latter on the sixteenth day after admission to hospital. Treatment directed to promote the excretory action of the kidneys, skin, and intestines, proved, as it too often does in such circumstances, of no avail.

There is, however, one aspect of this group of cases which is not without its therapeutic lesson. Allowing that the secondary inflammations which characterize such cases are excited by effete products which the kidneys have failed to remove from the blood, it is still possible that other influences, such for example as exposure to cold or damp, may have some share in their production. Certain it is that however produced, inflammations of mucous and serous membranes are apt to prove disastrous events to any patient the subject of chronic renal disease. A condition which, for instance, would, in a healthy man, be a simple bronchial catarrh followed by prompt resolution under some mild and simple treatment, may readily, in a patient with a damaged kidney, be the immediate prelude to a fatal issue. Hence an important element in the treatment of the victim of chronic kidney disease is to secure his protection against circumstances which tend to determine these, for him, disastrous complications. This no doubt involves attention to the activity of the organs engaged in elimination. It is, indeed, in this direction much more than in attempts to reduce the loss of albumin —an ambition probably beyond the resources of art—

that the pathway of safety is to be found. At the same time, it is reasonable to believe that those ill-defined influences which are covered by the term " chill," may, in the chronic albuminuric, as in the healthy—probably, indeed, more readily than in the healthy—cause pleurisy or bronchial catarrh or other similar events. It thus becomes a duty to warn such patients against these influences, and to indicate suitable measure of climate, clothing, and general hygiene, by which the necessary protection may be obtained.

The two cases here reviewed show with painful distinctness how subtle, how serious, and how largely beyond therapeutic control, are the various inflammatory developments which may occur in the course of chronic renal disease. They also enforce the duty of so guiding those who are the victims of this disease, that the risks to which such patients are in a special degree liable may be reduced to the lowest level that circumstances permit.

A CLINICAL LECTURE ON A CASE OF PERIPHERAL NEURITIS WITH ŒDEMA.

GENTLEMEN,—This patient—a man aged 35—has come to seek medical advice, because, during the last fourteen days, he has been troubled with pains in his lower limbs below the knees. These pains, it would seem, have not been very severe, yet they have caused the patient a certain amount of annoyance, and, as he puts it, they have "led" to weakness and to swelling of his feet and legs. The weakness has expressed itself in the form of occasional unsteadiness of gait, and once or twice his knees have suddenly given way and he has found himself sitting on the pavement. He appears to regard this latter development mainly from a jocular standpoint, and altogether speaks of his condition as though it had never given him much mental concern or anxiety.

The most instructive method of studying the case will be, I think, to consider carefully the patient's three symptoms—pain, weakness, and swelling—with a view to frame a diagnosis which will include both these and any further facts that may be discovered on physical examination.

In the first place, then, it is evident, that the swelling is a reality, for there is manifest pitting on pressure over the dorsum of each foot and along the subcutaneous surface of each tibia; a like condition is found also over the spines of the sacral and lumbar vertebræ. The swelling is therefore due to œdematous effusion in these regions. It is advisable to emphasize this, because patients frequently complain of "swellings" which have no objective existence, at least in the shape of œdema, and therefore, unless examination detects pitting on pressure, it is not safe to conclude that a complaint of "swelling" necessarily means œdema. Presumably, local dilatation of the vessels in any part of the body, however produced, causes a subjective sense of tension, and in this way painful parts may feel to be "swollen" to the patient, although there is no manifest change to the eye of the physician. But in the present case, as we have already seen, there is no room for uncertainty. In the patient's lower limbs, and over the lower part of his back, swelling is undoubtedly present, and this swelling is due to œdema.

Now it is obvious that the œdema is a clinical fact which claims an explanation, and perhaps, as a matter of experience, we expect this explanation in a male adult to be afforded, most probably, by disease of the heart or kidneys. An examination of the urine, however, fails to detect anything abnormal, and, in particular, all the tests for albumin give negative results. Similarly, there is no evidence of valvular disease of the heart, and while, perhaps, a very critical examination may suggest a slight extension of the percussion dulness to the left, neither this, nor the fact that the pulse is somewhat compressible, implies a degree of cardiac dilatation capable of producing the œdema present in our patient. The condition of the lungs, also, as determined by physical examination, is normal; and therefore the œdema cannot be due to the obstructive influence of chronic bronchitis or emphysema on the right heart and the venous circulation. Examination of the abdomen, likewise, yields normal results. Hence there is nothing to suggest a tumour, or other cause of pressure on the large veins of the trunk, as a condition responsible for the œdematous state of the lower limbs and loins. In some cases œdema attends certain anæmic states which produce grave general disability and cardiac weakness, but the general appearance of the present patient, apart from the negative results of physical examination, forbids such an explanation in the present instance. And so exceptional a cause of œdema as trichinosis need hardly be here discussed, for there have been neither the intestinal symptoms which attend this condition, nor widespread pain in the muscles, nor fever. In short, in attempting to establish the cause of the œdema we have been compelled to exclude (1) those general diseases —leucocythæmia, pernicious anæmia, malignant disease, etc.—which depreciate the quality of the blood and the force of the heart, (2) pressure on the large veins within the abdomen, and (3) disease of the heart, lungs, and kidneys. That is, the attempt to utilize the fact of the œdema as a guiding line to a positive diagnosis, has not proved very helpful. It is necessary therefore to turn our attention to other aspects of the case.

A second symptom of which the patient complains is weakness in his legs. Yet, when the power of the various muscular groups is tested they all appear fairly strong. The quadriceps and the hamstring muscles in each limb are certainly normal, but there is possibly a certain degree of flabbiness and feebleness in the calf muscles, and also in the anterior tibial group. But even this is not very definite, though it is necessary to remember that as the abnormal condition (in so far as it exists) is presumably bilateral, there is no opportunity for making specific comparisons between the two limbs, and therefore a relatively slight deficiency may fail to be confidently appreciated. If there is any actual paresis, it is certainly quite of a minor degree. When, however, the patient is made to walk across the ward, there is no doubt of some disability in the motor apparatus. He walks, as you observe, with his feet wide apart, and his gait is deficient in confidence—he cannot, as he says, " trust his legs." Further, he is unable to stand steadily with his feet close together when his eyes are closed. It is therefore obvious, that whether there is or is not defective power, ataxia is certainly present. The patient is unable successfully to co-ordinate the several parts of his muscular apparatus for such definite purposes as walking and the maintenance of equilibrium.

Another very important fact which appears on examination is complete absence of each knee-jerk. This is a conclusion which must never be accepted without very careful testing. It is not sufficient merely to have the knees crossed and to strike the patellar tendon. If this is followed by the development of the knee-jerk, well and good; but if not, more severe tests must be applied. Hence we make this patient practise " re-inforcement "; that is, we direct him to hook the fingers of one hand within those of the other and to "pull hard." The influence of this procedure is to distract his attention from the lower limbs and to heighten, as it were, the tension of the whole nervo-muscular mechanism. Further, we deliver the stroke over the tendon, not through the clothing, but directly on the naked skin. Yet, when tested in this and in other ways no response in the shape of the knee-jerk can be elicited. We conclude confidently, therefore, that the knee-jerks are absent. Now a combination of ataxia in the lower limbs with absence of the knee-jerks, cannot, in a man approaching middle-life, fail to suggest the diagnosis of locomotor ataxia. Further, there is in the present case a complaint of pain in the lower limbs, though this has not exactly the shooting or lightning-like character which is the usual kind of pain in this disease. In addition, the state of the sensory functions of the skin below the knee, in each limb, might quite justifiably be quoted in support of a diagnosis of locomotor ataxia. There are, in this respect, defects and peculiarities of very considerable interest.

In the first place, in reference to the sensory defect, it is obvious that there is marked deficiency of the capacity to appreciate painful stimuli, for when a needle is run through a fold of skin, picked up from the surface of either leg, the patient neither shrinks nor complains of pain. There is, also, some deficiency of the thermal sense, as is shown by the patient's want of confidence in distinguishing between a hot and a cold sponge. But tactile sensation, on the other hand, though perhaps a little dulled, is much less depreciated than the painful and thermal forms of sensation. Such a condition of the sensory functions is not, perhaps, particularly suggestive of locomotor ataxia, but it is by no means out of harmony with this diagnosis. The disease in which this loss of the sense of pain and temperature, with retention of the tactile sense, is most conspicuously displayed, is syringomyelia. But in this disease there is a gradually increasing muscular atrophy, usually affecting the upper limbs; the course of the disease is a very chronic one; and the knee-jerks are apt to be exaggerated rather than lost. Hence, as a possible diagnosis in the present case, syringomyelia hardly needs to be discussed. The disturbance of the sensory functions in the parts below the knees, might, however, fairly enough be combined with the character of the gait and the absence of the knee-jerks in support of a diagnosis of locomotor ataxia.

But can the œdema be explained by this diagnosis? This is, perhaps, just possible, though the proposition is a doubtful one. Sometimes locomotor ataxia, as a result of some disturbance of the trophic nerve mechanism, leads to swelling and other changes in the joints, and with these there may be more or less subcutaneous œdema. Hence it is just conceivable that œdema might occur apart from the joint affections, and the statement has been made that this does occasionally happen. But it is certainly an event so exceptional, that unless the diagnosis is very secure and all other possible interpretations can be confidently excluded, locomotor ataxia cannot safely be considered as an adequate explanation of the presence of œdema.

Now we have taken pains here to exclude the usual causes of œdema, but before we accept the suggestion that the case is one of locomotor ataxia, and that the œdema is to be explained as part of this disease, it will be wise to endeavour to obtain, if possible, other facts in support of the diagnosis. In this quest, however, we do not meet with much encouragement. Thus, we find none of the ocular symptoms of locomotor ataxia: the pupils have a perfectly good light-response; the optic discs are not unduly white; the visual acuity is good; and the fields of vision show no evidence of the commencing peripheral contraction which is often an early sign of optic atrophy. Again, the patient denies, alike, any girdle-sensation, any disturbance of the functions of the bladder, and any modification of the sexual function; and on testing the trunk, there is not at any level a zone of deficient sensation such as may sometimes be observed comparatively early in the development of locomotor ataxia. If, therefore, this diagnosis is to be maintained, it must find its support solely in the character of the gait, the absence of the knee-jerks, and the abnormal sensory condition in the legs. These conditions, speaking generally, may be ample to support such a diagnosis, but in the present instance, the absence of confirmatory facts, and the presence of œdema, compel us to scrutinize them with an extra degree of care, and to inquire also whether both they, and the œdema, may not be more surely covered by some alternative explanation.

What are the results of such a scrutiny and inquiry? First, in reference to the loss of the knee-jerks and the character and degree of the anæsthesia, there is nothing more to be said: these are, without question, well within the province of locomotor ataxia. The gait is hardly classified with equal confidence. There is, it is true, an ataxic-like character about it, especially when the patient is asked to walk along a narrow line, but at the same time it scarcely has the "stamping" element often seen in locomotor ataxia. Again, there is probably some degree of weakness in the muscles below the knee, but neither this, nor the character of the pain, is particularly suggestive of locomotor ataxia. Altogether, therefore, there is room for hesitation before concluding that the present case is one of locomotor ataxia attended by a very unusual complication in the shape of subcutaneous œdema.

What suggestion then can be set in opposition to this diagnosis? Now it may be usefully remembered, that while evidences of nervous disease having a bilateral distribution suggest in the first place a lesion of the spinal cord—because here the two halves of the central nervous system are in close anatomical association and are likely therefore to suffer in common—there are certain other possibilities to be considered. And one of these is that the lesion producing the bilateral manifestations may be, not in the cord, but in the peripheral nerves.

Once grant the introduction into the blood of some poison capable of producing prejudicial changes in the peripheral nerve-endings, say in the skin and muscles, and there must inevitably result bilateral symptoms of sensory and motor defect.

In view of this it may be asked whether a diagnosis of multiple symmetrical peripheral neuritis will fit the facts of the present case. Obviously such a diagnosis will explain the absence of the knee-jerks, because it includes a breach of the reflex arc upon the integrity of which the occurrence of the knee-jerk depends. As a result of such a breach, the free and ready transmission of nervous impulses between the nerve centres of the spinal cord on the one hand, and the muscles on the other, is no longer possible, and the knee-jerk, therefore, cannot be elicited. Peripheral neuritis will account also for the sensory defect and for the pain, quite as well as, if indeed not better than, the alternative diagnosis of locomotor ataxia. There is, moreover, in our patient another sensory condition which peripheral neuritis readily explains—namely, some degree of muscular tenderness. It is by no means so marked here as in many cases, but quite beyond question a fairly firm grasp of the calf-muscles causes the patient an abnormal degree of discomfort. The absence of the knee-jerks, and the cutaneous anæsthesia, then, adapt themselves equally to either of the two diagnoses now under discussion, while the character of the pain, and the muscular hyperæsthesia—the latter especially—favour the diagnosis of peripheral neuritis.

Considering next the gait, it is manifest that peripheral neuritis may explain, too, the defect in this function, that is the ataxia. Between the spinal centres and the muscles, there is, it must be remembered, a double set of communicating nerve-fibres. Along one, pass efferent impressions which stimulate the muscles to contraction. The others convey afferent impressions from the muscles to the nerve-centres, and thus, as it were, by keeping the centres informed of the state and condition of the muscular system, enable the centres to adjust their activity to the degree required for the contraction of the several muscles engaged in the performance of various complex actions, such as walking, balancing, etc. Now a lesion which interferes with the endings in the muscle of those nerve-fibres which carry impulses from the spinal centres to the muscles, must necessarily cause a greater or less degree of muscular weakness; the impulses, in such circumstances, cannot fully reach and successfully stimulate the muscles. On the other hand, a lesion involving the peripheral nerve-endings through which impressions travel from the muscles to the spinal cord, must necessarily mean that the spinal centres are badly informed regarding the condition of the muscles, and hence they will fail to adapt their activity to the degree of muscular action needed for specific ends. In the first case, in short, there is muscular weakness or paresis; in the second, there is ataxia. A combination of these two symptoms, therefore, is specially suggestive of peripheral neuritis; and it is this combination which exists in the present case. In most cases of peripheral neuritis, perhaps, the muscular weakness is more marked than the ataxia, and is attended with considerable muscular atrophy. But one has only to suppose, that in the present instance it is the second group of fibres—the afferent fibres—which have mainly suffered, to have an explanation (1) of the predominating ataxia, (2) of the less marked evidence of muscular weakness and flabbiness, (3) of the pain from which the patient suffers, and (4) of the muscular hyperæsthesia. A review of all these facts would thus seem to justify the suggestion that they find their explanation in a diagnosis of peripheral neuritis rather than in one of locomotor ataxia.

There remains, however, the question of œdema. This may, as we have seen, be just possibly brought within the diagnosis of locomotor ataxia. Is it or is it not more easily accommodated in that of peripheral neuritis? There is little difficulty in answering this question in the affirmative. Indeed, in one disease distinguished by a peripheral neuritis, namely beriberi, œdema is a common and prominent feature. Another illustration may perhaps be found in chronic arsenical poisoning, where both peripheral neuritis and œdema (usually of the eyelids) may be seen. Alcohol, in this respect, stands in much the same position. Probably in all of these the explanation of the œdema is to be found in a neuritis of vaso-motor nerves exactly comparable to the neuritis which, in the nerve-endings in muscle, produces muscular weakness, atrophy, and ataxia; and in sensory nerves, pain and other disorders of sensation. It is certainly highly improbable that our patient is suffering from beri-beri, if only for the reason that he has never been out of this country. Equally, arsenical poisoning, without any history to suggest it, and in the absence of undue pigmentation of the skin and of keratosis of the soles or palms, need hardly be considered. But the possibility of alcohol cannot be so lightly dismissed. The patient is a commercial traveller, and he admits that he is in the habit of taking frequent "nips" of whisky, and that sometimes he has "a drop too much" both of beer and whisky. Without any departure from charity, therefore, we may assume that for some considerable time his habits in reference to alcohol have favoured the production of peripheral neuritis, and that the history of the case, therefore, tends to support this diagnosis.

Diphtheria, diabetes, and lead poisoning are other causes of peripheral neuritis, but they can all be excluded from the present case. Hence, when all the facts are reviewed, there can be little hesitation in departing from the suggested diagnosis of locomotor ataxia, and adopting that of peripheral neuritis. There is not the degree of muscular atrophy often seen in this latter disease, and the motor defect is less that of weakness than of ataxia. But as the case is not an advanced one, the absence of muscular wasting does not seriously militate against the diagnosis, and the predominance of ataxia can be explained in the manner already suggested (p. 376); a similar predominance is sometimes seen in other varieties of peripheral neuritis, as for example in that which follows diphtheria. The neuritis in our present patient apparently involves nerve endings in the muscles, in the skin, and in vaso-motor tracts, and so produces, respectively, disturbances in the motor, the sensory, and the vaso-motor functions. It is to the last-mentioned that the œdema is in all probability to be attributed.

The diagnosis of peripheral neuritis enables us to give a much more hopeful prognosis than had we been compelled to regard the case as one of locomotor ataxia; and the early stage of the case still further improves the outlook. By cutting off the supplies of alcohol, we are able to remove the cause of the disease, and this restriction, combined with rest, will doubtless be followed by a gradual return of the damaged nerves to a normal condition. The same influences will tend to get rid of the œdema, and as soon as the muscular tenderness has subsided—and almost at once, indeed, as this is relatively slight—strychnine and the adoption of massage may be commenced with a practical certainty that the patient will ere long be restored to good health.

A CLINICAL LECTURE ON THREE CASES OF TABES DORSALIS.

GENTLEMEN,—The three cases to which I wish to direct your attention this afternoon, though differing widely in the symptoms they present, are all, I believe, examples of one and the same disease—namely, tabes dorsalis. The justification of the diagnosis in each individual case, must, of course, be provided in detail, but, accepting it in the meantime, I present the three cases in series because they illustrate some of the many different modes in which tabes dorsalis may commence its clinical career. In one of these patients the complaint is solely of pain, and there is no other admitted disability; the second suffers from gradual failure of sight, and only allows other symptoms under some pressure; and the third is the victim of an attack of double vision, with a history of a similar attack some twelve months ago. Not one of these patients suffers from difficulty in walking, and in none of them is there appreciable ataxia either in the lower limbs or elsewhere. Yet, I believe after you have examined them, you will have no difficulty in agreeing with me that they all belong to the group of cases commonly termed "locomotor ataxia." Hence they support the selection for this disease of such a non-committal name as "tabes dorsalis" in preference to a term which merely describes an individual symptom, and a symptom, moreover, which, as the present instances show, is by no means always present.

The first case is that of a man aged 35. He complains that for about four years he has been troubled with attacks of pain; and these, it appears, have been variously interpreted as rheumatism or gout or neuralgia.

At times he is entirely free from pain, but on other days he suffers so much that he is quite unable to attend to business. Often he will go to bed in his usual health, but will shortly wake up "in great agony," and he may then be unable to get any rest during several hours. The pains are principally in the legs and feet, but more recently they have also been very troublesome at the back of the head and behind the ears. Sometimes, he tells us, they appear to be on the surface, at other times deep in the muscles. He does not admit that "lightning pains" would be a good term to apply to them, but says that the worst variety is "like a deep toothache." Except for these pains—and this is one of the important aspects of the case—the patient considers himself in good health. He has, he thinks, been "pulled down a bit" by the frequency and severity of the pain, but he is satisfied that if he could only get rid of this one annoyance he would enjoy thoroughly good health.

This conviction is supported by the man's general appearance, which is by no means that of an invalid. Further, the usual routine examination detects no sign of disease in either the thoracic or abdominal viscera, and, as you see, his gait is entirely undisturbed and without trace or suggestion of ataxia. Yet, if one follows out the reasonable plan of examining the nervous system, just as is the rule in reference to the other organs of the body, there is a prompt discovery of facts which leave no doubt of the existence of organic nervous disease. In such an examination it is at once obvious that there is a degree of inequality of the pupils, and that neither is quite circular—facts which are, perhaps, not positively outside the limits of the normal. But there is, also, absence of the pupil light-response, and this, without question, is a definitely abnormal condition. The pupils are of medium size, and in this respect, therefore, do not correspond exactly to the contracted pupil usually seen in tabes dorsalis. But the defect in the light-response, with the persistence of free contraction during convergence, puts the pupils decisively in the Argyll Robertson class, and suggests at once central degenerative nervous disease. All the other results of our clinical scrutiny of the nervous system are negative, until we come to the examination of the tendon-jerks, and here, whatever manœuvre we adopt, we cannot, in either limb, obtain even a trace either of the knee-jerk or of the heel-jerk. Absence of these jerks, Argyll Robertson pupils, and a history of attacks of severe pains extending over months or years, make certain the position that the case belongs to the tabes dorsalis group. And in accepting this conclusion we note with interest (1) the conspicuous complaint of subjective sensory phenomena, and (2) the entire absence of motor disturbance, so far at least as the patient's consciousness is concerned. Loss of tendon-jerks and defective light-response in the pupils, though in a sense motor defects, cause no appreciable disability, and are only recognized by a trained observer. Hence, though highly significant to the physician, they receive, as here, no attention from the patient.

The first observation suggested by a conjunction of events such as is above related, is, that the diagnosis of a case of this order may be readily overlooked, unless a routine practice of testing the nervous system is regularly cultivated and practised. It is only by such a method that we have been saved, in this case of tabes dorsalis, from contributing to one or other of the erroneous diagnoses with which the patient has already been supplied, namely, "rheumatism or gout or neuralgia."

Another remark suggested by the scanty motor disturbances exhibited by our patient, is, whether there exists the possibility of a still further reduction of the motor phenomena in tabes dorsalis, so that the sole, or almost the sole evidence of the disease may be that of pain. Sir William Gowers has described such cases and has applied to them the term "tabetic neuralgia" —cases, that is, in which the pains of tabes exist—and continue perhaps for years—without any ataxia, and with a normal or approximately normal condition of the knee-jerks. The necessity of keeping this possibility in mind, supposing it to exist, is evident, as ignorance of it may involve a serious error in diagnosis. Our present patient has hardly the extreme simplicity of a case of *pure* "tabetic neuralgia," though he comes very near it. Were it not for the loss of the tendon-jerks and the state of the pupils, he would fulfil the strictest definition of this condition. The practical lesson is, that in any case of recurring pain—more especially if of bilateral distribution—affecting the limbs, the trunk, or, as seen in the present patient, the head, the possibility of tabes dorsalis should be kept in view, and a careful examination should be made for confirmatory facts. Among such facts may be mentioned pupillary defects, lessened tendon-jerks, commencing optic atrophy, delayed appreciation of common sensation, lowered pain-sense, bladder disturbances, and perhaps also, a history of syphilis. In brief, then, this first patient is an example of tabes dorsalis existing for at least four years in the shape of attacks of severe pain, the pain being unaccompanied by ataxia, but found, on examination, to be associated with defective response of the pupil to light and loss of the tendon-jerks.

The second patient, a man aged 54, has been under frequent observation during the last three years. His story is that shortly before his first visit to the hospital he had a cold in the left eye, and noticed that the sight of this eye seemed "dull." Then, to his great surprise, he found, on covering the right eye, that his left eye was almost blind. Such a history is related, now and again, by a person who has had a defective eye for years, or even from birth, the blindness being only discovered when the patient, accidentally as it were, covers over the sound eye. But our present patient is certain that this is not the explanation in his case, as three years before the discovery of the defect he attended an ophthalmic hospital to get glasses for near work, and he remembers distinctly that with each eye, tested separately, he had quite good vision.

The first examination of the patient showed that the visual acuity of the left eye was reduced to the ability to count fingers at four feet; on the other hand, with the right eye he could read 6/6, and with the necessary presbyopic correction, J.1 at 12 inches. So far, therefore, the presumption seemed to be that we had to deal with a purely uniocular defect, and one, probably, of comparatively recent origin. The ophthalmoscopic facts apparently harmonized with this view: the left disc was unduly white, and had the slightly cupped appearance which is frequently seen in optic atrophy; the right disc, on the other hand, could not in any sense be pronounced abnormal. Hence it seemed possible that the case might be one of embolism or thrombosis of the retinal artery; or an example of retro-bulbar neuritis; or due to some other circumstance capable of producing a unilateral optic atrophy.

When, however, the visual fields were taken, the case assumed an entirely different aspect. The left field, as was to be expected, was extremely contracted (fig. 42). But a not less certain defect was found in the field of the right eye, that is, in the eye with a full measure of visual acuity and with an optic disc and fundus appearing normal to the ophthalmoscope. As is seen in the chart (fig. 43), there exists in the upper and outer part of the right field a sector-shaped defect, and with this is associated some general contraction of the field, more especially of the field for red.

Now it is true that want of intelligence, or nervousness, on the part of the patient, may result in the production of an inaccurate perimeter chart; in such circumstances the chart is usually affected—contracted for the most part—at all parts of its circumference. But a localized defect such as is here exhibited is almost certainly incapable of so simple an explanation. A sector-shaped defect in the visual field, in short, can hardly mean other than organic change, and, more especially when associated with undoubted optic atrophy in the other eye, must be taken to mean optic atrophy in an early stage. The perimeter, therefore, in the case of our second patient, proclaimed a defect which neither the test for visual acuity nor the ophthalmoscope

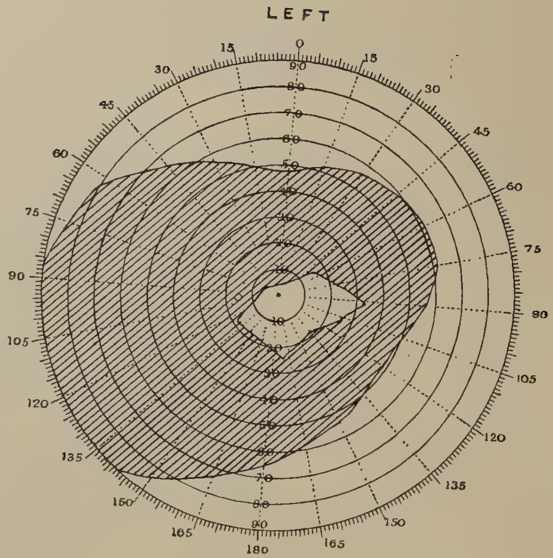


FIG. 42.—Contracted visual field showing small area within which "something moving" could be seen. No appreciation either of white or colour in any part of field.

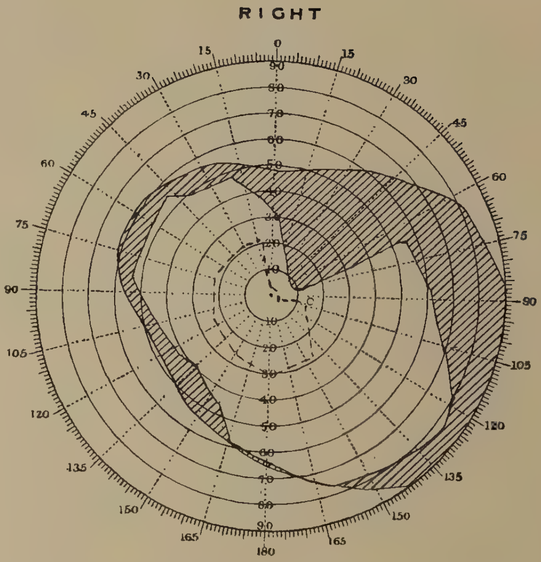


FIG. 43.—Visual field showing some slight general contraction and marked sector-shaped defect in upper and outer quadrant. The dotted line indicates the limits of the field for red. In central vision all colours (1 millimetre square) were promptly recognized.

had discovered, and by so doing, altered the entire complexion of the case. For, instead of a condition of unilateral optic atrophy, with the probability of a local and relatively insignificant explanation, we were forced to admit a bilateral atrophy, and, consequent on this, the practical certainty of a cause acting either centrally or through the agency of the blood-stream, and involving, in either event, the danger and risk of complete blindness.

But a case of bilateral optic atrophy is a case of nervous disease. Even if no other evidence of disease of the nervous system can be discovered at the time, such evidence will, sooner or later, almost certainly appear. And even occurring without confirmation elsewhere, a primary bilateral optic atrophy justifies a suspicion, to put it mildly, of changes in the nervous system of the tabes dorsalis order.

An examination of the present patient showed that evidence confirmatory of the diagnosis of tabes dorsalis was by no means wanting. In the first place there existed in each eye marked myosis, with the characteristic features of the Argyll Robertson pupil; secondly, the knee-jerks and heel-jerks were entirely absent; and, in the third place, in testing the sensation there was discovered a narrow zone of anæsthesia surrounding the trunk just below the level of the nipple.

All these facts, save and except the defective vision of the left eye, were, be it observed, non-existent so far as the patient's subjective experience was concerned. To him his disease was merely a defect in the vision of his left eye, just as in our first case the patient considered himself merely the victim of rheumatic or neuralgic pains. Yet in each, the diagnosis of tabes dorsalis is "writ large" for those who have eyes to see. Hence it is obvious, that a limited subjective complaint, and one by no means strongly suggestive of a central nervous lesion, may be the one and only symptomatic evidence of a disease which clinical examination readily recognizes as undoubted tabes dorsalis—that, in short, "neuralgia" existing alone, or optic nerve atrophy existing alone, may each be "tabetic" in nature and origin. Our second patient has now become almost blind. The clinical features above described remain without substantial change, and the optic atrophy has reached an advanced stage in each eye. Yet, he is still, it is to be noted, quite free from ataxia. The diagnosis of tabes dorsalis has, however, received further confirmation from the appearance of shooting pains in the lower limbs and of some difficulty in emptying the bladder. The case may, in brief, therefore, be accurately defined as one of tabes dorsalis with optic nerve atrophy as an early and predominating event, the optic atrophy having produced practical blindness in one eye before sensibly affecting the vision of the other.

The third patient is a man aged 44, and he, also, complains of a single symptom—namely, double vision. In all other respects he considers himself in good health. The cause of his diplopia is obvious at a glance, namely, a widespread paralysis in the area of distribution of the left third nerve. There is defective ability to move the globe inwards, upwards, or downwards, and with this is some degree of ptosis; the pupil is dilated and immobile, and the power of accommodation is lost—the latter condition being shown by the man's inability to read small type except with the aid of a plus 4.0 spherical lens.

Now an ocular paralysis may be an entirely local affair, and due to such a relatively trivial cause as cold or rheumatism. But it is an event which, like optic nerve atrophy, brings the nervous system in the widest sense under suspicion. An attempt in this patient to find further evidence to justify such a suspicion, results, however, in failure. To every one of the clinical tests used to determine the structural and functional integrity of the nervous system, there is a completely satisfactory response. In particular, the tendon-jerks, the visual acuity, the sensory function, and the visual fields, are all fully up to the normal standards. Hence, at first sight, it might appear to be a position capable of defence, to hold that this case belonged to the group of "simple" ocular palsies. But in no case of ocular paralysis can such a position be a very secure one until it is fortified by the lapse of time. Further, our present patient tells us that eight years ago he had a venereal sore, followed by a quite undoubted development of secondary syphilis. And not less significant is the fact that some twelve months ago he was under observation in consequence of a diplopia due to a paresis of the left sixth nerve. Now clinical experience shows, whatever be the explanation, that the pathological process in tabes dorsalis is apt to produce ocular paralyses; that these are often of comparatively brief duration; and that they tend to recur. The repetition of the palsy is, then, an unpleasant feature of the case. Having regard to this experience, it is impossible to take a light-hearted view of a case in which a transitory sixth nerve paralysis is followed, after an interval, by a paralysis of the third nerve. And the necessity for the more anxious prognosis is emphasized by the reflection that the patient is a man of middle age, with a history of syphilis quite without qualification or doubt.

But allowing that the ocular paralysis here is probably a consequence of syphilis, is it necessary to conclude that it belongs to the tabes dorsalis order, and that it carries the prognosis associated with this disease? For syphilis, as is well known, may produce an ocular paralysis directly, that is, by a specific meningitis or gummatous deposit involving the trunk of one or more of the cranial nerves supplying the muscles of the eyeball, and such a condition may be capable of a fairly confident cure by the appropriate remedies. In such circumstances, however, we expect a complaint of headache, often severe, and especially at night; and perhaps, also, optic neuritis, or other evidences of intracranial disease. But in the present case there is no such evidence. Further, we know that syphilis, as a matter of fact, is often responsible for the degenerative processes in the nervous tissues which form the pathological basis for the disease known clinically as tabes dorsalis. Therefore it is impossible to avoid the fear that it is in events of this latter order that the interpretation of our patient's recurring diplopia will probably be found; and this, even though no other evidence of nervous disease can at present be discovered.

Granted this conclusion, the case affords an illustration of a third manner in which tabes dorsalis may exhibit its earliest clinical manifestations. And once recognize that an ocular paralysis, with its resulting diplopia, may be the first evidence of tabes dorsalis, and it is obvious that no other symptom of the disease is more likely to come promptly under the notice of the physician. A patient may suffer a considerable amount of pain without either feeling much alarm, or seeking medical advice. He may even, as we have seen to-day, be the victim of some degree of optic atrophy—so long at least as this is confined mainly to one eye—without being convinced that there is anything very serious at fault. But a diplopia is a highly inconvenient and alarming event, and urges the most phlegmatic patient to seek guidance and treatment. Hence, should tabes dorsalis commence in this fashion, it is not altogether strange that in such a case definite confirmatory evidence should be lacking. Such a consideration must influence our judgment in the present instance, and in view of this, of the history of syphilis, and of the repeated ocular palsies, we are, I think, bound to regard the case as one of ocular paralysis appearing as the first event in the clinical evolution of tabes dorsalis.

Assuming then that the diagnosis in each of these three cases is justified, we see in the first of the series tabes dorsalis displayed mainly, and indeed from the patient's point of view, solely, in the form of pains in the limbs and head; in the second case, an optic nerve atrophy with the resulting defect of sight is the sole complaint; while in the third, a recurring diplopia is the one and only sign that the degenerative processes of tabes dorsalis are at work in the nervous system.

A word may be said on the treatment of these cases. The first patient—that is the man with tabetic neuralgia—has for the last three weeks been taking chloride of aluminium, the remedy introduced by Sir William Gowers for tabetic pains. In addition, as soon as he is conscious of the approach of an attack of pain, he takes ten grains of acetanilide and lies down for at least half an hour, this latter condition being of decided value in securing the full action of the remedy. Further, every night at bed-time a hot-water douche is applied to the spine for ten minutes, and once a week he has a mustard pack along the dorsal and lumbar spine. These latter measures may, perhaps, seem to be without reasonable basis, seeing that the pains are excited, in all probability, in the peripheral nerves, not in the central nervous system. Even so, it is not impossible that changed conditions of circulation and nutrition in the spinal centres may influence the peripheral nerves. Certainly patients often find these local measures to be of distinct value, and this, after all, is the last word in any question of therapeutics. To whatever element in this scheme of treatment the improvement is due, the patient himself has no doubt that he is decidedly better, the pains being both less frequent and less severe.

The patient with ocular paralysis, when first seen, was brought fully under the influence of mercury and potassium iodide, on the view that the condition might, perchance, be due to a gumma or to syphilitic meningitis. But as these measures, after a three weeks' course, produced no improvement, they were discontinued, and arsenic and nux vomica were prescribed, together with the local application of electricity. Whether as a result of these measures, or merely as a coincidence, the sixth nerve paralysis soon began to improve, and the patient remained well until a few days ago, when, as we have seen, a third nerve paralysis developed. He will now return to the tonic treatment, which he probably discontinued far too soon. It is treatment of this order, rather than large doses of mercury and potassium iodide, which is required by the degenerative nerve lesions following specific disease. At the outset, as here, if there is any possibility of a specific deposit or inflammation as the cause of the symptoms, it is right to give a short and energetic course of anti-syphilitic treatment. But if this does not soon produce the desired result, it is useless, perhaps even worse than useless, to continue it, though this is not to say that small doses of mercury, in conjunction perhaps with more definite tonic agents, are not of value in the treatment of tabes dorsalis. So administered, at least at the outset of the disease, mercury may indeed render distinct therapeutic service.

What has just been said may be repeated with regard to the treatment of the patient with optic nerve atrophy. This, in cases of tabes dorsalis, is a degenerative process, the indirect result of the syphilitic or other toxin, and not an outcome of a definitely specific lesion. Vigorous anti-syphilitic treatment, in my experience, never does these cases any good, and I question whether it does not sometimes do harm. Improvement, in some instances, is produced by the hypodermic use of strychnine, but to secure this the drug must be given in full doses—that is, in doses which produce physiological reaction, and the capacity of different individuals in this respect varies within wide limits. You can hardly push the remedy to its full extent unless you have the patient under continuous observation, and hence, in out-patient practice, this plan of treatment hardly gets a fair chance. But, in more than one case, I have seen the sight improved by it, though I must admit the improvement was but for a season. Altogether, tabetic optic nerve atrophy is an unhopeful condition; yet, if noted early, strychnine, as above described, and combined with full doses of cod liver oil and other tonics, may do something to stay its progress.

A CLINICAL LECTURE ON TWO CASES OF TABES DORSALIS IN WOMEN AND ON A CASE OF JUVENILE TABES DORSALIS.

GENTLEMEN,—We have here to-day, at the outset, two quite undoubted cases of tabes dorsalis, and the first point of interest they provide is that in each instance the patient is a woman. It is one of the puzzling etiological relations of tabes that men, so much more frequently than women, are its victims. Assuming, as is generally admitted, that syphilis, directly or indirectly, is the cause of the disease, one would have anticipated that the distribution of tabes dorsalis in the two sexes would have been fairly equal. Yet there are not wanting other conditions, undoubtedly syphilitic, in which parallel peculiarities of sex-distribution exist. Thus, syphilitic ulceration and stricture of the rectum are much more frequent in women than in men, and Sir Jonathan Hutchinson has pointed out that in the hereditary variety of syphilis, iritis, and in a less degree keratitis, are more often seen in female than in male children. The peculiarity of sex-distribution in tabes dorsalis, therefore, though mysterious enough in all conscience, by no means stands alone; parallel experiences are to be found both in syphilis and in many other forms of disease. Little is known to account for such inequalities in the sex-distribution of disease, and an investigation covering the entire pathological and clinical field in this respect would form a very interesting inquiry.

Tabes Dorsalis: Examination of Cerebro-Spinal Fluid.

Our first patient is 40 years of age, and she came to hospital complaining of flatulence and discomfort after food. Her gait was then noticed to be ataxic; examination showed the pupils to be unequal, with absent light-response; and the tendon-jerks in the lower limbs could not be obtained. These facts leave no room for doubt in reference to the diagnosis, but as a matter of interest the cerebro-spinal fluid was examined, and in this there was found a considerable increase of the cellular elements. This method of examination has distinctly increased our diagnostic range in tabes dorsalis and in general paralysis. While normally the fluid is almost free from cells, in each of these diseases, on the other hand, the cellular elements in the fluid are more or less numerous. In such a case as the present, the diagnosis, quite apart from the condition of the cerebro-spinal fluid, is only too obvious. But in an early and doubtful case, where, perhaps, there is but a single symptom—say, for example, an ocular paralysis, some disturbance of the bladder, or pains in the limbs—the examination of the cerebro-spinal fluid is a method of very high value.

Tabes Dorsalis; Optic Atrophy; (?) Juvenile Tabes.

Our second patient is 29 years of age, and is unmarried. As you see, she has a highly ataxic gait, and indeed so tumultuous is she in this respect, that, although she has plenty of muscular power in her limbs, she cannot walk without support. Neither the knee-jerks nor the heel-jerks can be obtained. The left pupil is distinctly larger than the right, and neither the one nor the other shows any light-reflex or contracts in the act of convergence. In this latter respect, therefore, the pupils depart from the typical Argyll Robertson pupil, but such an experience in tabes dorsalis is not, after all, a very uncommon one. The usual condition of the pupils in tabes is that they are contracted, perhaps unequal, with no response to light but with free movement in the act of convergence. As here, however, the latter movement, as well as the light-reflex, may be absent, and occasionally the pupils are dilated rather than contracted.

A second feature of interest in this case is discovered when the visual functions are tested. The ophthalmoscope shows that in each eye the optic disc is very pale, and one begins, therefore, to think of the possibility of optic nerve atrophy—a well-recognized event in not a few cases of tabes dorsalis. But it is never safe to adopt this conclusion merely because the disc appears to be unduly white. Normal discs differ widely in tint, and now and again, what appears at first sight to be an atrophic disc is found on further examination to possess a full measure of functional value. When our patient's sight is tested, it would seem as if the present case afforded an example of the truth of the proposition just stated. For, in spite of the decided pallor of the disc, the visual acuity, as you see, is 6/6 in each eye. Central vision is, in other words, of the normal standard. But a very different position emerges when the peripheral visual capacity is tested. The visual fields are decidedly and unduly limited, as appears in the perimeter charts (fig. 44), and this, added to the ophthalmoscopic examination, is more than sufficient evidence of the existence of optic nerve atrophy. Thus the present case affords an illustration of the truth that good—even normal—central vision is perfectly compatible with the existence of a considerable degree of optic atrophy. If reliance, therefore, is placed merely on the ordinary estimate by test types, a very erroneous conclusion may be formed. To avoid such an error, it is necessary to examine, as by the perimeter, the functions of the peripheral portions of the retina. Then, as in the present instance, conclusive evidence of optic atrophy may sometimes be obtained, even in a case in which central vision satisfies the ordinary standards of the test types.

First, therefore, in connection with the present case, we may note the existence of tabes dorsalis in a woman; secondly, the loss of the pupillary movement in convergence as well as its absence under the stimulus of light; and thirdly, the peripheral contraction of the visual fields, as an expression of optic atrophy, with retention of normal central vision.

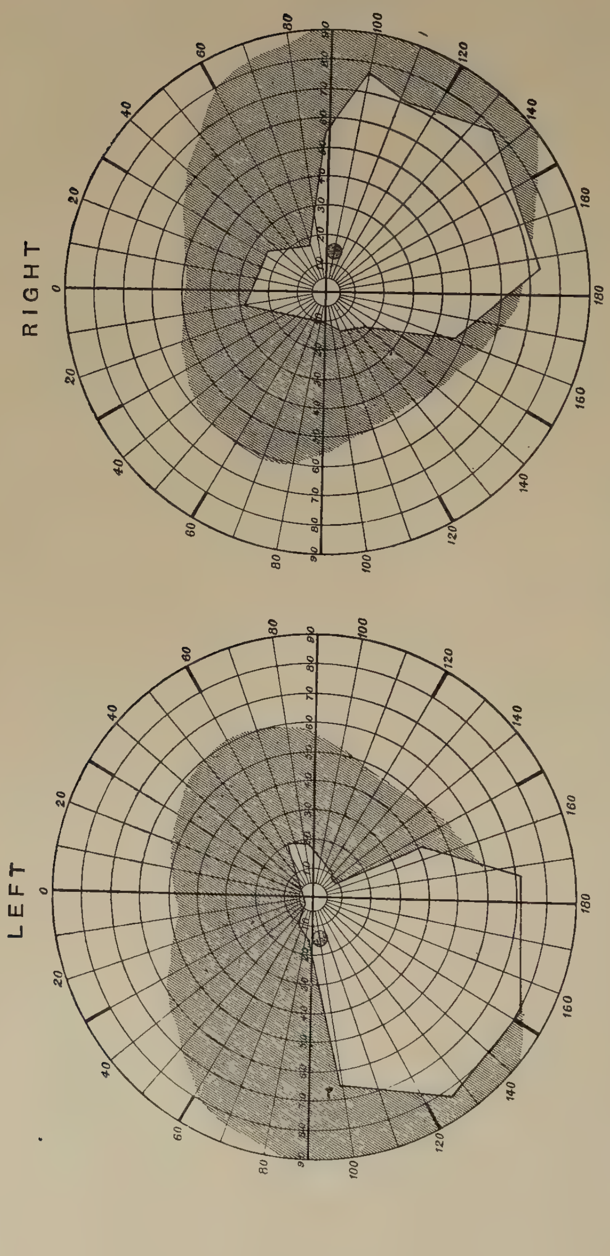


FIG. 44.—Perimeter charts showing considerable peripheral limitation of each visual field in a patient the subject of optic nerve atrophy, but with central vision of normal standard ($\frac{6}{6}$ in each eye).

There is, however, a still more noteworthy feature in this case. As already stated, the patient is unmarried and is only 29 years of age; and she has had symptoms of the disease for about twelve months. Now for tabes dorsalis to commence under thirty years of age is decidedly uncommon; and when the disease is associated with a history of syphilis, its development is usually separated from the primary infection by an interval of many years. Whether this patient has had acquired syphilis or not, we cannot say with absolute certainty. But there is nothing in the history to suggest it, nor is there the slightest evidence of the disease on examination. It is fair, therefore, to suggest that the balance of probability is against it. On the other hand, it is not without significance that the left upper central incisor has a distinct notch in its free border. Does this mean hereditary syphilis? Perhaps it is a slender basis on which to rest so large a diagnosis, but there can be no doubt, first, that signs of hereditary syphilis may be strictly limited to the teeth, and secondly, that in hereditary syphilis, although—if the teeth are affected at all—both incisors are usually notched, it does occasionally happen that the notching is asymmetrical (fig. 45). If to these two propositions we add a third, namely, that tabes dorsalis has occasionally been observed in young patients bearing signs of inherited syphilis, the possible importance in our present patient of (1) the "notched" upper central incisor, and (2) the absence of all evidence of acquired syphilis, becomes manifest.

The case, I venture to suggest, is probably an example of "juvenile" tabes dorsalis due to inherited syphilis. Other active manifestations of inherited syphilis, as for example interstitial keratitis, may, as is well known, be postponed until the second or third decade, and therefore the relatively late appearance of signs of tabes dorsalis does not necessarily exclude the diagnosis here proposed. What is certain, is that this woman aged 29 has tabes dorsalis; that she is free from all evidence of acquired syphilis; and that she shows some, though only slight, evidence of the inherited disease.

Can anything further be done to advance the diagnosis beyond this position? We have examined the cerebro-spinal fluid and found some, though only a slight excess of lymphocytes, and the Wassermann reaction, we are told, is negative. Still the fact remains

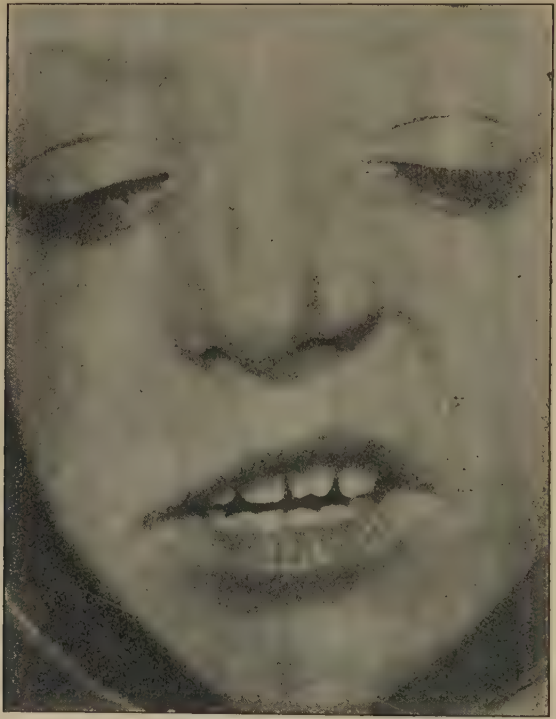


FIG. 45.—Case of inherited syphilis as shown by interstitial keratitis, and scars at angles of mouth. The photograph illustrates the statement that in inherited syphilis the "notching" may be limited to a single tooth.

that the woman has tabes dorsalis, and that in all probability, therefore, she has at some date or other been infected with syphilis. The results of the two tests just recorded do not get rid of these conclusions. Hence the only question is whether the case is one of acquired or inherited syphilis, and, for reasons already given, the balance of evidence seems in favour of the latter. Examination of the parents or of the brothers and sisters, if this were possible, might help the situation by the discovery of conclusive signs of specific taint in the family record. In the present instance these investigations are not possible; but it is worth remembering that this line of inquiry may be very helpful in a doubtful case of inherited syphilis; moreover, the discovery may be made in some unexpected direction. Thus, in a child or adult, perhaps apparently healthy, a disseminated choroiditis may in such circumstances as we are now considering be highly significant, and when, as may sometimes happen, the choroidal disease has a purely peripheral distribution, the individual concerned may be entirely free from any suspicion that he is the subject of disease. Therefore, in doubtful cases of inherited syphilis, the clinical rule ought to be to examine the other members of the family, not forgetting in this examination the use of the ophthalmoscope.

Perhaps it is worth while asking whether any diagnosis other than tabes dorsalis could be applied to the present case. Obviously peripheral neuritis would explain the absence of the tendon-jerks and perhaps also the ataxia, but it would not account for the pupil defects nor for the optic atrophy. Hereditary ataxia (Friedreich's disease) is a rare, though well-recognized, cause of ataxia and loss of knee-jerks in young persons, and while its clinical exhibition is usually first noted in childhood or at puberty, it is sometimes postponed until the twenties have been entered. But Friedreich's disease is not marked by loss of the pupil light-response nor by optic atrophy, while, on the other hand, it is usually associated with nystagmus, with defective articulation, and with a history showing the existence of the disease in several members of the family; and none of these conditions exists in our present patient.

There is one respect in which the present case follows the course of hereditary ataxia rather than that of tabes dorsalis—namely, the absence of pains from the ataxic limbs. The patient's own statement is that "some twelve months ago the right leg and foot, and later the left leg, began to get stiff and drawn at the toes, and after this the knees got weak and began to give way"; but she denies anything like acute or severe pain. Usually, in tabes, when there is ataxia and loss of knee-jerks, lightning pains in the lower limbs are more or less troublesome, whereas in hereditary ataxia such pains do not occur. But this single negative experience, namely, the absence of pain from the ataxic limbs, cannot outweigh the evidence in the opposite direction, and therefore we may confidently recognize our present case as one of tabes dorsalis possibly, or even probably, associated with inherited syphilis.

Juvenile Tabes Dorsalis: Ocular Paralysis.

This patient is a lad aged 17, and here, whatever may be thought of the case we have just considered, the diagnosis of juvenile tabes dorsalis, will, I think, be generally admitted. The patient himself asserts that he is in good general health, and his one complaint is, that when looking upwards he "sees double." The explanation of this disability is on the surface, for, on testing the ocular movements, it is at once evident that there is a slight but quite distinct defect in the upward excursion of the right eyeball. The lad is, in short, the victim of an ocular paralysis. Hence there is an obvious claim for a complete examination of his nervous system. The result of this examination is negative, with, however, one highly important exception, namely, absence of both knee-jerks and of both heel-jerks. Possibly, in some very exceptional instance, such absence is natural to the individual; or, to speak more correctly, a patient is occasionally met with in whom the tendon-jerks are absent though no other suggestion of organic nervous disease can be discovered. Even this throws some measure of suspicion on the integrity of the nervous system, and it may be that time alone, in any individual instance, will resolve the doubt. But when, in addition to the absence of the tendon-jerks, there is also an ocular paralysis, the case is not one of suspicion but of certainty. An ocular paralysis may perhaps have a "simple" explanation, and possibly, though rarely, absence of the tendon-jerks may carry but little clinical import. But the two together leave no doubt: the combination, without question, means organic disease. Thus the diagnostic problem presented by this lad is to find some organic disease of the nervous system possible at 17 years of age and capable of explaining both an ocular paralysis and the absence of the tendon-jerks.

Perhaps, in such circumstances, the first suggestion which occurs to the mind is diphtheritic paralysis. In this, there is often, or indeed usually, loss of the tendon-jerks; and sometimes such loss is the sole evidence of diphtheritic paralysis. Moreover, an ocular paralysis may be a feature of the disease. It is true that the patient denies all history of sore throat, but, as many experiences teach, the local manifestations of diphtheria may be so slight as to be overlooked, and yet the subsequent paralysis may be severe. Once more, examination detects the presence of albumin in the urine; and this, again, is a not uncommon result of diphtheria. There thus seems to be a plausible case for a diagnosis of diphtheritic paralysis. But there is something to be said on the other side. First, the absence of all history of sore throat or other illness, though not, perhaps, absolutely excluding the diagnosis, certainly does not favour it; in the second place, while diphtheria not infrequently paralyses the internal musculature of the eyeball (the ciliary muscle in particular, and sometimes the sphincter iridis) it rarely paralyses the extrinsic muscles; and again, one of the commonest exhibitions of diphtheritic paralysis, namely, paralysis of the soft palate (evidenced by a "nasal" quality of the voice and by regurgitation of fluids through the nose) is not present, nor is there anything in the history to suggest that it has ever been present.

All these considerations seem to form a serious obstacle to the diagnosis of diphtheritic paralysis, and consequently, possible alternative explanations of the facts of the case come into prominence. One of these, namely, Friedreich's ataxia, though perhaps accounting for the absence of the tendon-jerks, can be dismissed for reasons given in the consideration of the previous case (p. 397). A second, juvenile tabes dorsalis, is not so readily put aside. Loss of tendon-jerks, and ocular palsies, are well-known features of acquired tabes dorsalis, and equally, they are known in the much less common and "juvenile" form of the disease. In this regard it has to be noted that the patient shows none of the stigmata of hereditary syphilis. Yet, when we examined his cerebro-spinal fluid, we found an excess of cellular elements, and his blood-serum, as Dr. Torrens reports, gives a positive Wassermann reaction. The case for juvenile tabes dorsalis due to inherited syphilis is thus rendered a very strong, if not a conclusive one, and this, be it observed, in a patient whose one and only complaint is double vision, and who is entirely free from all the more obvious features of specific disease.

The above position was the one reached on the first examination, and now, six months after the first observation, there is still further evidence in favour of the diagnosis of juvenile tabes dorsalis. The original conditions remain, but in addition, the right pupil no longer responds to light, and shows but slight movement during convergence; and both in Romberg's test, and when the lad tries to walk along a straight line, there is distinct evidence of ataxia in the lower limbs. In the face of these developments, the diagnosis will, I think, be generally allowed.

With regard to the specific element, we cannot get much help from the family history. The lad is an orphan, and he has only one sister. But this girl, unfortunately, cannot be examined, as she lives at a distance. It is, perhaps, significant that she has had "trouble with her eyes," and that she is deaf, as interstitial keratitis and nerve deafness (possible causes of these defects respectively) are common events in inherited syphilis. But even without any assumptions in this direction, the clinical record of the patient affords an ample basis for the diagnosis.

A word may be added in reference to the albuminuria. There is every reason to believe that this is a "functional" condition, or at least that it is not due to organic renal disease. The centrifuge yields neither tube casts nor renal epithelial cells; there are no cardio-vascular changes; the ophthalmoscopic examination is negative; and the sphygmomanometer gives, not a high, but a low maximum reading, namely, 95 mm. Hg. Hence there cannot be in the kidney the fibrotic changes which indicate interstitial nephritis, and this is here the more important seeing that for such changes hereditary syphilis has sometimes been held responsible. Whether such an accusation is or is not well founded is a difficult question, but it does not touch the present issue. The chief interest of the case is the progress of the diagnosis from what seemed at first sight to be a "simple" ocular paralysis, to a conclusion so serious as juvenile tabes dorsalis.

A CLINICAL LECTURE ON SOME CASES OF SYPHILIS IN MARRIED WOMEN.

GENTLEMEN,—The principal interest of the three cases which we have to consider to-day relates to the question of diagnosis. In each, the diagnosis I propose to suggest is that of acquired syphilis, though it must be admitted the evidence is limited in amount, and, at least at first sight, is capable of misinterpretation. Each of the patients is a woman, and thus there are obvious objections to anything like a searching or suggestive inquisition into the personal history. Of the primary infection it is hopeless to expect information, for, other reasons apart, this, in women, as everyone recognizes, often fails to attract attention. It may also be accepted that so far as inquiries may venture, there is in none of the patients any history of events which may be marked confidently, or even suspiciously, " secondary " syphilis.

Assuming the diagnosis to be correct, the important clinical doctrine which these records illustrate and enforce, is, that syphilis in married women often runs a latent course, and that its manifestations may be restricted, perchance, to a single event. This limitation, together with the absence of anything like a distinctive history, makes an oversight in diagnosis comparatively easy. Therefore it is important to bear in mind the doctrine just stated, and cases which illustrate it are worthy of attention.

Subcutaneous Gumma without other Evidence of Syphilis

The first patient is a healthy-looking woman, aged 40. Her sole complaint is of the existence in the upper part of her left shoulder of a painful lump, which she has noticed during the last seventeen days. There is a considerable swelling over the border of the upper part of the left trapezius muscle, and the skin is much inflamed. At first the condition was regarded as an inflammatory thickening and likely to terminate in an abscess, but under full doses of potassium iodide the "lump" is rapidly disappearing, although at one point there has been a slight leakage of thin purulent matter. There can be little doubt that the swelling was a gumma tending to break down, and the rapid improvement under potassium iodide confirms the diagnosis. It is not very infrequent to find such localized gummatous conditions to be, in married women, the sole evidence of syphilis. Quite recently at hospital I have seen one such swelling over the patella, and another on the posterior aspect of the lower end of the humerus, the patient in each instance being free from manifest syphilitic incidents; in each case the condition was probably a specific periostitis, and it rapidly improved under potassium iodide.

Gumma of Liver: Suggestive Child-bearing Record.

In our second case the suggestion is that the patient has a gumma in the left lobe of the liver. She came to hospital complaining of great pain and a sense of "dragging in the stomach." Examination readily detected a firm mass in the left hypochondrium, tender to palpation, and descending with the descent of the diaphragm in inspiration. There could be little doubt that this was the enlarged left lobe of the liver, and though now less conspicuous, it can still be readily recognized. An enlargement of the liver limited to the left lobe, it was thought, was not likely to be malignant disease, although such a limitation is not impossible; and hepatic abscess in a patient who had never been out of this country was felt to be highly improbable. Hence, with these excluded, there came the diagnosis of syphilis as the probable explanation of a limited and non-uniform enlargement of the liver;

and the cessation of the pain and diminution of the swelling under potassium iodide makes this suspicion very near a certainty. The patient's child-bearing record gives some help towards the same conclusion: she is the mother of ten living children, the youngest being 10 years of age; and since the birth of the youngest child there have been three other pregnancies, all of which have terminated prematurely.

Jacksonian Epilepsy: Perforation of Palate.

Our third patient is a woman aged 26, and while there are events in the history, and also existing facts, much more decisive than anything found in the two previous cases, the diagnosis presents aspects which are not in some of its forms in married women. The patient tells us that for twelve months before she came to without interest in relation to the recognition of syphilis hospital she suffered from "epileptic fits," and that she has been under treatment for these, but without much benefit. When asked to describe one of the "fits," she says it commences with "a nerve" in her right hand and then passes along her upper limb and extends to the face; she is, during the attack, quite conscious of what is going on, but is unable to speak. Sometimes, she says, she has a "stronger fit," in which she falls to the ground and becomes unconscious; in none of the fits has she bitten her tongue or lost control over the bladder. Manifestly such a description as this compels a suspicion of Jacksonian epilepsy rather than of the ordinary idopathic disease. The uniform commencement of the seizure in one and the same place, and the extension from the site of origin along a definite path in harmony with the anatomical arrangement of the "motor" centres in the cortex of the brain, are exactly what is to be expected from a localized lesion affecting the Rolandic region of the cerebral convolutions. No doubt it is often said that in Jacksonian epilepsy the seizure is not attended by loss of consciousness; and generally this is true. But in a severe attack the convulsions may spread over the whole of the body, and in these circumstances the patient often or even usually becomes unconscious. It is, of course, not uncommon after a Jacksonian seizure for the patient to recognize more or less weakness, sometimes amounting to actual paralysis, in the affected limbs, but in the present instance, while the patient affirms that her hand becomes "numb," she does not allow that it is ever "weak." There is, however, enough in her description to justify an extremely strong suspicion of Jacksonian epilepsy, and the question therefore arises whether any evidence of a cause for this condition can be discovered, it being, of course, acknowledged, that a localized syphilitic development or gumma is the most likely explanation. And examination, even apart from the history, makes it practically certain that she has had syphilis. Thus, in the soft palate is a circular perforation, while the front of the right leg shows a scar with a considerable amount of pigmentation, and although other explanations of these appearances are, perhaps, possible, syphilis is much the most likely. There is no history of skin eruption or of loss of hair, but patient admits that some years ago she had a "bad throat," and after this an "ulcerated leg"; also that shortly before the "fits" commenced she suffered for some time from "horrible headaches." She has been married nine years and has had two pregnancies, the first terminating in the birth of a dead child. Altogether there cannot be any reasonable doubt of the true nature of the case, and the point of interest and importance is, that a married woman who complains of "epileptic fits" is found to be the subject of intracranial syphilis, the demonstration, in this respect, being completed by the statement that both fits and headaches have promptly ceased under the influence of anti-syphilitic treatment.

A CLINICAL LECTURE ON SOME CASES OF SYPHILITIC PERIOSTITIS.

GENTLEMEN,—The cases which we have to study to-day are clinical illustrations of the various circumstances in which periostitis occurs in the course of syphilis. The series is more particularly of interest as showing that this expression of specific disease may be encountered in many stages and phases of the malady. Thus, in one case the patient is a child, manifestly the subject of the congenital form of syphilis; in a second case, the patient, a woman, has a periosteal node associated with the presence of a secondary skin symptom. Two other cases are examples of periostitis in the late stage of syphilis; and in the sixth patient a localized periosteal thickening has appeared on each tibia a year or more subsequent to a diagnosis of tabes dorsalis, that is, periostitis is found with a condition recognized to be post-syphilitic rather than a direct result of a definitely specific lesion.

Periostitis of Ulna and Tibia—Interstitial Keratitis.

This patient is a girl aged 12. She was brought to hospital complaining of pain in the right arm and leg. The cause of this is at once manifest on examination. The upper half or so of the subcutaneous surface of the ulna is much thickened, and towards the proximal end the bone is so much enlarged that it forms a considerable globular tumour. A similar thickening is present over the subcutaneous surface of the right tibia, so that this is rendered decidedly convex; the left tibia shows the same features, but in a less conspicuous degree. These facts are hardly open to more than one interpretation, and any doubt about the diagnosis is at once dispelled by further examination. In the first place the child has obvious caries of the nasal bones, as shown by flattening of the bridge of the nose and by a highly offensive discharge from the nostrils. Again, there is a history of a severe inflammatory affection of the eyes some few years ago, and on focusing the cornea with a powerful convex lens, fine branching lines can be traced in its substance, these showing the former existence of an interstitial keratitis. The diagnosis of congenital syphilis has, therefore, a more than abundant support. The child has much improved under treatment by potassium iodide and syrup of iodide of iron. In particular, this treatment has promptly relieved the periosteal pain and swelling.

Secondary Syphilis—Periosteal Node on Rib.

The evidences of secondary syphilis in this patient, a woman aged 30, are quite beyond question. There is a fairly copious skin eruption over the trunk and limbs; and each tonsil is enlarged and shows a patch of superficial ulceration. With these evidences of the so-called secondary stage, there is a considerable fusiform swelling on the second right rib, tender to touch, and causing the patient more or less pain. All these symptoms are attributed by the patient to a recent attack of influenza, since which she has had "pains all over." Such pains are, of course, not uncommon in the early stage of syphilis, but it is quite uncommon to see evidence of a localized periostitis in the form of a node, while, at the same time, the patient shows the presence of a secondary skin eruption.

Periostitis of the Tibia and Fibula—Evidence of a former Iritis.

This patient, a man aged 57, complains of pain in the lower part of his left leg, of two months' duration, and especially troublesome at night. The lower part of the leg is seen to be much thickened, so that roughly it is as thick again as the opposite limb. The skin is somewhat reddened, and there is a moderate degree of pitting on pressure. In addition, considerable thickening of the lower ends of the tibia and fibula is readily appreciated. Patient is said to have had "gout" some eight years ago, but he denies having had either a chancre or any evidences of syphilis; he describes himself as the father of five healthy children. Examination showed some defect in the light-response in one part of the circumference of the left pupil, and now, after the instillation of a drop of a solution of homatropine, the pupil is seen to be markedly irregular in consequence of numerous posterior synechiæ. When questioned on this point, the patient remembers that he had "rheumatism or gout in his eyes" about eight years ago. The value of a careful examination is thus illustrated by the discovery of a fact, the evidence of a former iritis, which helps to approve the diagnosis.

Periostitis of Tibia—Aortic Disease.

The necessity for a complete examination of the patient, even when the diagnosis appears to be written on the surface, is illustrated, also, in the case of this man, aged 45, who, like the previous patient, complains of pain in the left lower limb, shown on examination to be due to a periostitis of the tibia. There is a history of a venereal sore followed by a skin eruption some twenty years ago. Thus the diagnosis seems, without further examination, to be complete, and all the more so that the patient, save for the pain as above described, regards himself as in good health. Yet, on placing the finger on the radial artery, a marked collapsing quality of the pulse is at once appreciated. And physical examination of the præcordial region shows conspicuous evidence of aortic disease, including a loud diastolic murmur heard with special distinctness over the lower sternum. The noteworthy fact is that the presence of aortic regurgitation, even in an extreme form, is seen to be consistent with the profession of good general health. The patient has been a sailor, and afterwards a member of a fire brigade, and it may be assumed that the strain associated with these occupations, and the influence of the syphilitic poison, are jointly responsible for degenerative changes in the aorta, and that these changes have extended to and rendered incompetent the semilunar valves. The general examination of the patient has thus resulted, not only in the discovery of a fact which confirms the diagnosis, but also in the timely protection of the patient against a danger which, but for the examination, would not have been apprehended. In this, and also in the previous case, potassium iodide in 10-gr. doses produced rapid improvement both of the pain and of the periosteal thickening.

Tabes Dorsalis followed by Nodes on each Tibia.

This case presents several features of interest. In the first place, the patient, a woman aged 35, is undoubtedly the subject of tabes dorsalis. For more than five years she has complained of pains in the limbs and trunk, coming on suddenly "like an electric shock." With these, on frequent occasions, there have been associated attacks of agonizing pain in the abdomen, attended with vomiting and great prostration—doubtless gastric crises. The next development was the formation of an ulcer on the right sole, followed after a considerable interval, by a similar condition in the left foot; and this condition has caused her much pain and disability. Examination shows the pupils to be small, unequal, and destitute of light-response; the left knee-jerk cannot be obtained, and that of the right side, though present, is defective; and on each side the heel-jerk is absent. A history of "electric" pains and of gastric crises, associated with perforating ulcer of the sole, with Argyll Robertson pupils, and with defective tendon-jerks, makes escape from a diagnosis of tabes dorsalis impossible.

Mainly in consequence of the condition of her feet the woman was admitted to hospital, and while there she has developed over the crest and inner surface of each tibia, 3 in. or so below the insertion of the ligamentum patellæ, a considerable swelling, tender to the touch and without question of periosteal origin. The symmetry of the arrangement is very conspicuous. The patient suffered much pain in connection with the development of these nodes, and this was only subdued by large doses of potassium iodide. The chief interest of the case is, that in a patient the subject of a degenerative nervous disease regarded as an indirect and remote result of syphilis, the virus should still possess sufficient activity to be able to produce lesions of specific character. Taken together with the other cases, this record shows, that though convenient and capable of justification, the conventional division of syphilis into precise stages, each marked by features peculiar to itself, is not in absolute correspondence with the facts. Hence, while certain general statements are true, exceptions to them must be recognized. Periostitis, for example, is seen, as in the above records, not to be restricted to one special phase of the disease, but to occur, at least in occasional cases, at all stages of its progress.

A CLINICAL LECTURE ON THE OPHTHALMOSCOPIC FACTS IN CASES OF ALBUMINURIA GLYCOSURIA CONGENITAL SYPHILIS AND CHLOROSIS.

GENTLEMEN,—The first three cases which I have brought here for discussion this afternoon, possess in common a feature of much clinical interest and importance, namely, the presence in the fundus oculi of certain appearances which lend material assistance to the attempt to frame an accurate diagnosis. Possibly in every one of these cases a correct diagnosis could have been reached apart from the use of the ophthalmoscope. But the process would have been more tedious, and the result a less confident one. Indeed, it is safe to go further and to say that in each of these patients neglect to examine the fundus oculi might readily have been followed by a complete failure to apprehend the true nature of the case. On the other hand, so soon as the ophthalmoscope is used, we are at once introduced to pathological changes of such significance that they either at once conclude the diagnosis, or indicate unmistakably the line of investigation which it is necessary to pursue. Parallel experiences are by no means infrequent, and this is hardly a matter for surprise when it is remembered that the ophthalmoscope gives access to a class of information which cannot be obtained by any other clinical method. Short of the use of the X-rays, the clinical examination of the thoracic and abdominal viscera—and much the same is true of the nervous system—is an indirect, not a direct one. But the ophthalmoscope enables us actually to see both the peripheral expansion of a cranial nerve and a complete scheme of arterial and venous distribution, as well as a number of other complex and highly-organized tissues. All these parts are, of course, subject to the pathological misfortunes that pertain to the tissue-systems to which they severally belong, and the evidences of these misfortunes may often be readily recognized by the ophthalmoscope; and this, too, before, and somtimes long before, proofs of the existence of disease can be obtained from any other source. Further, the anatomical and physiological relations of the various parts of the eyeball to the brain and its related structures, mean, that the existence of many forms of intracranial disease may be recognized by pathological changes in parts that fall within the scope of ophthalmoscopic examination. Hence the range of this method of examination is a very extensive one. It covers not only local disease of the ocular structures, but also many changes which signal either some general constitutional disorder, or the existence of disease in some more or less remote organ.

The cases we have now to examine are illustrations of the truth of these statements, and it is mainly with this purpose in view that I present them. Before considering them in detail, it is of some importance to note that none of the patients, when they first came under observation, made any complaint whatever of defective vision, and therefore there was, on the surface at least, no particular claim for an examination of the fundus oculi. None the less, as you will see, there is in each case undoubted disease of the retina and its related parts. One of the clinical lessons which these cases enforce, therefore, is that highly significant facts may be discovered by the ophthalmoscope, even though the patient regards his vision as normal (and perhaps rightly so), or at least does not complain of any disability in this respect.

Frontal Headache—Retinis—Albuminuria.

This patient is a man aged 40. He first came under observation about eighteen months ago, when his sole complaint was of frontal headache; this attacked him every day after the mid-day meal and persisted until bedtime. As he had long worn glasses for myopia, he thought that some deficiency in this respect might be responsible for the headaches, and accordingly he consulted Mr. Brittin Archer at the Central Ophthalmic Hospital. Mr. Archer found the glasses the patient was wearing to be in every way suitable, but he discovered, on ophthalmoscopic examination, the presence of a number of highly refractive dots in the immediate neighbourhood of each yellow spot. Further investigation detected the presence of a small quantity of albumin in the urine, of a high-tension pulse, of distinct evidences of arterial degeneration, and a loud ringing quality of the aortic second sound. These observations leave no doubt that the patient is the subject of chronic renal disease, probably in the form of chronic interstitial nephritis, and the lesson now to be emphasized is that the detection of this condition was a direct result of ophthalmoscopic examination. It is no doubt true that an examination of the urine would doubtless have led to the same conclusion, just as it is true that the wise plan is to make such an examination in every case, whether the symptoms do or do not suggest disease of the kidney. But in an imperfect world it is easy to imagine this patient being treated in various ways with a view to relieve his headache, while the essential nature of the case was never even suspected. From such a misfortune, as you see, the ophthalmoscope provided a sure deliverance.

The ophthalmoscope did something more than this, for the facts detected by its aid indicated the necessary line of treatment, and now, as a result of suitable dietetic and hygienic measures, with regulation of the bowels and the occasional use of small doses of blue pill to reduce undue arterial tension, the patient has lost his headaches, and is, he considers, in the enjoyment of good health.

There is still, however, albumin in the urine, and it may be that restoration of the kidneys to a normal condition is impossible. But with proper supervision and regulation of his habits, there is no need why this man should not enjoy a long period of good health and activity. He is, admittedly, in a position less secure than that of a perfectly healthy and structurally sound man, but this defect can, at least in some measure, be met by suitable care. It is no doubt true that in many, perhaps in the majority of cases of albuminuria, the development of retinitis is of serious prognostic omen. But this is because such a development is usually a late feature in the evolution of the disease. Here it appears to be a relatively slight and early feature, and so far from progressing it has distinctly receded. It cannot, I think, be held, that in such a case as this present one, the existence of retinitis necessitates an extremely bad prognosis. And here is the fact, that it has without question existed for eighteen months, while the patient's general health is decidedly better than when he first came under observation : the retinitis has not advanced, and has produced no visual disability. The point, however, which I wish to emphasize, is that it was the use of the ophthalmoscope which, in a case liable to be misread, gave an immediate key to an accurate diagnosis and guidance to an appropriate scheme of treatment.

Chronic Dyspepsia—Retinitis—Glycosuria.

This is another patient whose case enforces a similar clinical lesson. She is 55 years of age, and some months ago she came to the Royal Waterloo Hospital with a complaint of weakness, flatulence after taking food, and constipation. In short, so far as her own story was concerned, her case did not materially differ from the numerous examples of chronic dyspepsia that are to be met with in every hospital out-patient department. But the ophthalmoscope at once detected numerous white dots and patches, with a few small hæmorrhages, scattered widely over each fundus. This, of course, led to an immediate examination of the urine, and thus to the detection of the presence of sugar. The amount was relatively small—about five grains per ounce—and as the patient denies that she suffers from thirst, or has to rise at night to pass urine, it is not probable that there is much, if any, polyuria. But the absence of these symptoms makes all the more emphatic the value of a routine use of the ophthalmoscope. For had there been marked general evidences of diabetes, the claim for an examination of the urine would have been an obvious one. In this instance, also, there was therefore a ready danger of neglecting an important pathological condition, and for preservation from this danger we were indebted to the assistance afforded by an ophthalmoscopic examination.

Notched Upper Central Incisors—Disseminated Choroiditis.

Our third patient is a young girl who came to the hospital complaining of pains in her limbs and "general weakness"; but with these symptoms we need not in the meantime concern ourselves. In the course of the examination it was observed that there was a small but distinct notch in the free margin of each upper central incisor. This necessarily raised the question whether the patient was the subject of congenital syphilis. Now, it is natural, I think, to shrink from so serious a diagnosis as congenital syphilis when only a single piece of evidence can be advanced in its support, and especially when, as here, such evidence is not altogether free from ambiguity. The girl's teeth are certainly notched, but not in the conspicuous and unmistakable fashion which one often sees. In any event, it must certainly be desirable to attempt to strengthen the diagnosis by collecting further evidence. This end is here immediately secured by means of the ophthalmoscope. In each fundus there are numerous scattered patches of choroiditis, but as none of these is situated in the immediate neighbourhood of either macula, there is no serious prejudice to the patient's power of central vision. The discovery of the choroiditis, added to the "notching" of the upper incisor teeth, makes certain the diagnosis; and this discovery, it is to be noted, was not prompted by any complaint of defective vision, but was due solely to the cultivation of a routine ophthalmoscopic examination.

There is yet another piece of evidence which the ophthalmoscope secures for us in this case. If a high plus lens is used, so as to focus the cornea, there will be seen a number of fine tortuous lines in the corneal tissue. These are, beyond doubt, the remains of vessels, and they prove conclusively the existence of a former interstitial keratitis. Thus any hesitation which attached itself to the slightly-marked notching of the teeth, is dispelled; and the case, as a result of examination by the ophthalmoscope, stands revealed as a manifest example of congenital syphilis.

It may be worth while adding, that in some instances, where there is hesitation in framing a diagnosis of congenital syphilis because of the doubtful evidence supplied by the individual patient, an examination of other members of the family may successfully resolve the doubt. Recently I have seen at hospital a child with obscure evidences of organic cerebral disease and without any of the recognized stigmata of congenital syphilis; on examining one of the other children, however, I found a well-marked disseminated choroiditis. This carries a practical suggestion, which, in doubtful or obscure cases, may be of some value.

The present patient has one further feature of interest, inasmuch as she justifies the good prognosis which is usually given in reference to interstitial keratitis. It is certain that she at one time suffered from this condition, yet now, it is only by very careful examination that any defect can be recognized in either cornea.

Optic Neuritis in Chlorosis.

In the next three patients we have further illustrations of the importance of ophthalmoscopic examination in medical cases. These patients illustrate, also, some of the clinical associations of optic neuritis, as this occurs in chlorosis. Without detailed examination for the moment, I will ask you to accept it as entirely beyond question, that after careful and repeated observations it is not possible to discover in any one of these three young women any evidence of disease other than chlorosis. Yet it is not less certain that in each of them there either is, or has been, a condition of double optic neuritis.

The first patient has only recently been admitted to hospital, and the optic neuritis is in the acute condition. You can see that each disc is very much swollen and that it shows considerable exudation with numerous hæmorrhages. Yet, when the patient is asked to read the test types, you find that she has apparently a full measure of vision, and this, both for distant and near objects. Here, then, is a conspicuous demonstration of the doctrine that double optic neuritis may exist without any appreciable impairment of vision. The patient, in this instance, it is true, does complain that her sight is "misty," but in other cases, even with an equal degree of optic neuritis, there may not be the slightest suspicion in the patient's mind that any abnormal condition of the visual apparatus exists. It is therefore only by using the ophthalmoscope that these cases of optic neuritis in chlorosis will be discovered, at least in the early and curable stage. The proportion of such cases in relation to the total number of patients who suffer from chlorosis, is doubtless a very small one. But every now and then, if one is on the look out, an example of this condition will be discovered.

The discovery of optic neuritis in chlorosis is important in two different directions: in the first place, because it calls for thorough and decided treatment; and in the second, because there is danger of interpreting the optic neuritis as significant of a disease much more serious than chlorosis, namely, intracranial tumour.

The importance of early and energetic treatment in cases of this order is emphasized by a comparison of the visual efficiency in the next two patients. One of these is a young woman who first came under observation some three years ago. From the history of the symptoms it was manifest that she had been anæmic for several months; and she admitted, that though she had been ordered to take iron, she had not done so with anything like regularity. On examination there were discovered manifest evidences of double optic neuritis, but a neuritis no longer in the acute stage; on the contrary, the neuritis was declining, and with this there was some organization of the inflammatory exudate and consequent pressure on the nerve fibres, producing, as a result, a degree of optic atrophy. In short, the position was one of post-neuritic optic atrophy rather than of optic neuritis; there were, also, in each eye, distinct evidences of retinitis. It was therefore not surprising to find that the girl's power of vision was prejudiced. She could only see on the distant test types part of the 6/12 line, and though under treatment which cured the chlorosis the vision improved to a slight extent, it did not by any means reach the normal standard of acuteness, and as you now see, the defect persists. There is doubtless some degree of permanent damage to the conducting power of the optic nerve fibres, and we can therefore now hardly hope for any further improvement in the patient's vision. Thus, while it is quite true that double optic neuritis does not necessarily imply visual defect during the active stage of the neuritis, there is definite risk of injury to vision (optic atrophy) as the inflammation declines. The danger therefore is, that if a patient with chlorosis only comes under treatment when she is sensible of failing vision, the mischief is already done. Hence to use the ophthalmoscope in chlorosis only when complaint of defective sight is made, is to run the risk of missing the optic neuritis at a stage when almost certainly the condition is susceptible of a complete cure.

Contrast in respect to the visual efficiency the position of the patient just described with the third patient of this series. This young woman came to hospital some nine months ago; she was decidedly anæmic, and was found to have a severe double optic neuritis, and with this, in her case, there was some depreciation of the visual acuity. Now she enjoys good general health, and her vision is fully up to the normal standard. And, still further, all traces of the neuritis have disappeared, so that even the most critical examination of each fundus fails to detect anything on which to found a suspicion of the existence of optic neuritis either now or at any previous date. In this case treatment was early and vigorous; the patient was kept in bed and iron was ordered in full doses. The result is a normal degree of vision and the removal of the neuritis without injury to the capacity of the optic nerve and retina. All this is very different from the second case, where treatment had been delayed and inefficient, and where, as a consequence, there is a definite injury to the sense of sight.

It is therefore obvious, that when optic neuritis is present in a case of chlorosis there is an added necessity for thoroughness in treatment. The basis of the therapeutic scheme, of course, must be iron in full doses. In addition, I think it is always well to keep the patient in bed until the optic neuritis has definitely subsided. Complete rest is a valuable agency in all cases of anything like severe anæmia, and even in ordinary examples of chlorosis it is well to order the patient to retire at an early hour. I feel sure that many cases of chlorosis are prolonged because such a direction is not enforced.

When chlorosis is complicated by optic neuritis there is an additional reason for insisting on complete rest. The immediate cause of the neuritis is uncertain, but in my judgment at least, there are good grounds for believing that it is due to thrombosis in one or more of the cerebral veins or sinuses—a development which undoubtedly occurs in occasional cases of chlorosis. If this is so, it is obvious that there may be a possible detachment of a portion of the thrombus, with a risk of pulmonary embolism, and to avoid this complete rest is imperative.

Another aspect of optic neuritis in cases of chlorosis is, as already indicated, the possibility that the discovery of the neuritis may give rise to the suspicion of a cerebral tumour. And this is all the more likely, seeing, as Sir Wm. Gowers has pointed out, that headache is a common symptom in chlorosis, and gastric disturbance with vomiting is not very unusual. If such a diagnosis is made, and a chlorotic patient is, as a result, treated with potassium iodide instead of iron, there may be very unfortunate consequences in respect to the function of the optic nerves. Hence it is necessary to remember that headache, vomiting, and optic neuritis, may all be due to chlorosis. At the same time, it must not be too readily concluded, when optic neuritis is present with evidences of anæmia, that the neuritis is necessarily due to the anæmia. Obviously a patient may be both anæmic and the subject of an intracranial tumour. On the other hand, as has been shown on more than one occasion, optic neuritis has occurred in conjunction with an event so suggestive of organic intracranial disease as an ocular paralysis, and yet all the symptoms have disappeared as the anæmia was cured by the administration of iron. These experiences, on the one side and the other, show, that when optic neuritis, with or without retinitis, is found in a patient showing signs of anæmia, the diagnostic position is far from being a simple one, and the question of prognosis is one of great delicacy. Perhaps the best general suggestion to offer is to say, that while remembering that the anæmia may be the explanation of the optic neuritis, this explanation should only be accepted after all other possible causes have been excluded. One such cause particularly to be borne in mind is cerebral tumour. Another is renal disease. There is a form of renal disease which, like chlorosis, is specially prone to occur in young women, and which may exist entirely without dropsy or other suggestive symptoms, the first indication of the disease being either headache, or some visual defect associated with optic neuritis or retinitis. Hence, when either of these last-mentioned conditions is discovered, it is not safe promptly to conclude that an existing anæmia is an adequate explanation. Rather it is only after several complete examinations of all parts of the body, after repeated testing of the urine, and after the establishment of a confident negative in both of these respects, that one is justified in concluding that optic neuritis or retinitis has its explanation in anæmia and that a good prognosis may therefore be given. Even then the opinion must be held with a certain amount of reserve until it is confirmed by the success of the treatment already indicated. At first sight it may seem somewhat startling to suggest that cases of cerebral tumour and cases of chlorosis may at times be mistaken the one for the other. Nevertheless, the statement is true, and indeed in some instances a confident distinction between the two conditions is only possible after repeated examinations and when the lapse of time has given an opportunity for the effects of treatment to be added to the other facts necessary to be considered in deciding the question at issue.

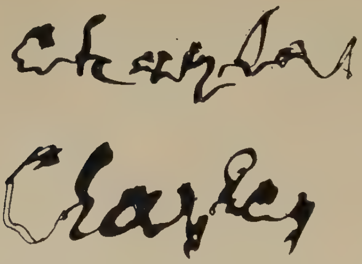
A CLINICAL LECTURE ON SOME CASES OF DISSEMINATE SCLEROSIS.

GENTLEMEN,—The clinical picture of disseminate sclerosis to which we are introduced in systematic works on medicine may be compared to a composite photograph. Such a remark applies, more or less, to the organized descriptions of all diseases. The picture is obtained, not from the lineaments of any individual case, but by a junction or fusion of the various appearances observed when a large series of cases is passed in review. Hence no individual patient can be expected to present all the features found in a systematic description of the disease of which he happens to be a victim. It must be sufficient for the diagnosis that the symptoms actually present are those of the disease in question, and that the combination of positive facts is such as to render any alternative interpretation highly improbable. This general rule applies with particular force to disseminate sclerosis. For, as the pathological pattern of the disease varies over a wide range, so also necessarily does its clinical expression. The small patches or "islands" of sclerosis which constitute the pathological anatomy of the disease, though distributed for the most part in the motor tract in the central nervous system, are not, in different cases, arranged on a uniform plan, but occur in unequal distribution and at various levels. Nor are they by any means invariably restricted to the motor tract. Again, in one case the "islands" may exist principally in the brain, in another in the spinal cord, whilst in a third they may be scattered at random, so to speak, through the whole length of the cerebro-spinal axis, and may, perhaps, be lodged also in some of the peripheral nerves. Hence the clinical picture presented by one patient may be very different from that presented by another. And, from individual cases, what are regarded as almost distinctive or classical signs and symptoms of the disease, may be altogether absent. It is these considerations which, in a special sense, render the clinical study of a large number of cases of disseminate sclerosis highly desirable. There is so wide a variety in the modes in which the disease is exhibited, that unless many examples are studied, the preparation for its recognition in ordinary clinical work is apt to prove inadequate.

We have to-day to examine a series of cases, and we shall see in them illustrations of some of the many types in which disseminate sclerosis exhibits itself.

Extreme and Widely-spread Intention Tremor; no Nystagmus.

This patient is a man of 31 years. As he enters the room it is seen that he has a stiff and extremely unsteady gait. He also has marked jerking movements in the upper limbs, and these are very evident when he uses a walking stick in the endeavour to steady himself. Tested in the usual fashion by directing the patient to touch the tip of his nose with his index finger, "intention tremor" is very conspicuous, and the same symptom is prominently exhibited in the handwriting. When the patient is seated it is easy to appreciate tremor of the head, caused directly by the muscles of the neck and not merely communicated from the considerable jerking movements of



Handwriting of Patient with Disseminate Sclerosis.

the limbs. This, also, it may be noted, is "intention tremor," as with the trunk erect, the muscles of the neck are active for the purpose of securing equilibrium; and the tremor ceases when the patient lies down. Other facts in the diagnosis are the prompt and explosive character of the knee-jerks, and loss of control over the action of the bladder. Hence there can be no doubt that the patient is the subject of widely scattered lesions in the central nervous system and affecting mainly the upper motor neurons—that, in short, he is the subject of disseminate sclerosis. Yet, in spite of this conclusion, there is not even a trace of nystagmus, and the patient's speech is not abnormal. Further, neither ankle-clonus nor Babinski's sign can be obtained. Thus the case illustrates the proposition that the diagnosis of disseminate sclerosis may be beyond question, and yet some of the most distinctive clinical features of the disease may be entirely absent.

Diplopia, Ataxic Gait, Exaggerated Tendon Phenomena, Drawling Speech and Nystagmus.

Here again the most conspicuous defect in the patient, a man aged 36, is an unsteady, straddling gait. There is also tremor of the head when he endeavours to stand at "attention," but the upper limbs show no appreciable intention tremor. On the other hand, not only is there marked exaggeration of the knee-jerks, but in each limb ankle-clonus and Babinski's sign are readily obtained. Nystagmus appears somewhat doubtful until the eyes are examined by the ophthalmoscope, when a very fine rhythmical tremor is appreciated. Slight nystagmus, which may escape ordinary observation, may be detected in this way, as the ophthalmoscope magnifies the fundus, and of course therefore and in proportion any tremor of the eyeball. The most prominent fact about the eyeballs, however, is paresis of the internal movement of each globe, with resulting diplopia. Again, the speech is decidedly peculiar. It is hardly a typical example of "staccato" speech, but has a measured, "drawling" character, with a decidedly monotonous intonation. The patient's wife noticed the development of this peculiarity some three years ago, and it, together with some unsteadiness of gait, was the first sign of the disease. That is, a symptom which is not present at all in the first case is here one of the earliest developments. Further, while this second patient suffers from diplopia, he has no trouble with his bladder; the first patient, on the other hand, suffers from incontinence of urine but knows nothing of double vision.

Diplopia with Exaggerated Tendon Phenomena, but without Tremor or Nystagmus.

This patient, too, is a male, and his age, 33 years, is within the period at which disseminate sclerosis is most common, that is, from 20 to 40. He illustrates very conspicuously that an ocular paralysis may not only be one of the events in the development of the disease, but that it may be the first seriously to attract the patient's attention. In the present instance the patient had complained of occasional headache and some giddiness for a few weeks, when suddenly he found himself "seeing double," the explanation being an obvious paresis in the outward movement of the right globe. There is no obvious defect in the gait or in the movements of the upper limbs, but each knee-jerk is in excess, and ankle-clonus and Babinski's sign are present on each side. Further, the patient admits that for some months he has readily become tired on walking, and that at times his right leg after exertion began to shake. These are, not infrequently, early symptoms in cases of disseminate sclerosis and ought always to suggest the possibility of this disease.

Optic Nerve Atrophy as apparently the Earliest Sign of Disseminate Sclerosis.

This patient is a man, aged 23, who, in his own view, has one ground of complaint and one only, namely, defective vision—a condition which has been gradually increasing during the last twelve months. Of the visual defect there can be no doubt, as the patient can barely see 6/60, and of the near types nothing less than J20.

The discs are extremely white, and are without question atrophic. Further, the usual clinical examination of the nervous system, in spite of the absence of complaint by the patient, at once detects evidence of widespread disease in addition to optic atrophy. Thus the knee-jerks have both an explosive character and an exaggerated excursion; in the left limb a sustained ankle-clonus is readily provoked; and in the right, though clonus is not so easily excited, its rhythmical character and its persistence leave no practical doubt that the condition is abnormal. On neither side can Babinski's sign be obtained. There is a degree of swaying in Romberg's test, as also some tremor of the head and of the left upper limb. No quite definite nystagmus is present, but in many extreme positions of the eyeballs there is some coarse jerking, which in some situations has a rhythmical character; and with the ophthalmoscope fine oscillations of the eyeballs can be appreciated. Of these various abnormalities the patient, of course, has no knowledge; for him his disability is purely and solely one of defective sight. Yet the examination shows that the condition, namely, optic nerve atrophy, on which this defect depends, is merely one incident in a series of pathological changes in the nervous apparatus, and that the case, which at the outset appeared to be one purely of optic atrophy, is really one of disseminate sclerosis with optic atrophy as an early and prominent development. It is well known that such atrophy is a not infrequent occurrence in the course of disseminate sclerosis, but it is unusual to find it in so advanced a form when the other features of the disease have not yet attracted attention.

Another aspect of the case is that the patient used to be a heavy smoker, using, according to his own statement, three-quarters of an ounce of shag daily. This is, of course, a quantity and quality of tobacco calculated to cause tobacco amblyopia. But against the suggestion that tobacco is responsible for the visual defect in this case is the patient's age. He is only 23, and tobacco amblyopia is not usual until middle life is approached or reached. Again, he has given up smoking for several months and without any material improvement in his vision. Then the optic disc is uniformly white, as in a general atrophy, and the signs of degeneration are not restricted to the lower and outer quadrant, as is common in tobacco poisoning.

Perhaps also it should be added that the patient is by trade a painter, and that lead has, in some instances, been responsible for optic atrophy. But this man has never had any symptoms or signs of lead poisoning, and in view of this, of his age, and of the other circumstances of the case, it would be unreasonable to regard the atrophy of the optic nerves as the result of the toxic action of lead. In addition, there are, as we have seen, distinct signs of disseminate sclerosis —a disease in which optic atrophy is known to occur. Hence it may be held that this diagnosis covers all the facts, the only peculiarity being the early and advanced atrophy of the optic nerves relative to the other features of the disease. Whether tobacco or lead, or the two poisons conjointly, have had any share in determining this order of events it is impossible to say.

Paresis of Right Limbs with Exaggerated Tendon Phenomena on Both Sides.

In this case (the patient, a woman aged 31) there is a very different clinical picture from any of the preceding cases. The most prominent symptom is weakness amounting almost to complete paralysis of the right lower limb. According to the woman's story the weakness has been developing " for years," though its more extreme form has only been present for some six months. Examination shows also distinct relative deficiency in the grasp and other movements of the right upper limb, so that at first sight the condition appears to be one of hemiplegia. But, as distinct from hemiplegia in its usual form, it is to be noticed that the muscles are free from rigidity and contracture; and, though such a distribution is not conclusive, the lower limb is considerably more paretic than the upper.

Further, there is no history of any cerebral attack attended with loss of consciousness or with sudden paralysis; the muscular weakness, indeed, has been a very gradual development. This latter feature sometimes marks the course of a hemiplegia due to an intracranial tumour, but our present patient has never suffered from headache or vomiting, and there is no optic neuritis. On the other hand, there is a much exaggerated knee-jerk; rectus-clonus and ankle-clonus are present; and these phenomena are as conspicuous in the quasi-normal as in the paretic limb. Babinski's sign is not obtained on either side. Now some exaggeration of the knee-jerk may, it is true, be detected in an ordinary hemiplegia, and in the sound as well as in the paralysed limb. But such extreme excess of the tendon phenomena as is present in this case in the apparently unaffected limb (left) can hardly be explained as a mere disturbance accompanying a right hemiplegia. Hence, taken with the history of the case, with the facts above related, and with the admitted experience that paralysis of one or more limbs may occur in the course of disseminate sclerosis, the tendon phenomena confirm the diagnosis, and this in spite of the absence of nystagmus, slurred speech and other symptoms.

Disseminate Sclerosis in a Patient in whom the Disease Existed in a Severe Form Sixteen Years Previously.

In this case we are fortunate to possess a history which is guaranteed by exact medical observation. And the history is one of the most instructive and interesting features of the case. The patient, a woman, is now 47 years of age. Her gait has a somewhat "shaky" character, and there is an obvious measure of spasticity in the movements of her lower limbs. All the tendon-jerks are considerably exaggerated, and though ankle-clonus is not obtained, Babinski's sign is elicited in each foot. Further, there is manifest nystagmus in each eye—vertical in the right, rotatory in the left. Hence there can be no hesitation or difficulty in accepting the diagnosis of disseminate sclerosis. The woman herself has no anxiety about her condition, and indeed, she considers herself—and with justice as the history shows—to have recovered from the worst features of her malady. It appears that fully sixteen years ago she was a patient in a hospital devoted to the reception and treatment of diseases of the nervous system, and the details of her condition at that time are fully described in the hospital journals. Shortly before admission she had gradually lost power in the right lower limb, this being followed by a somewhat sudden paralysis of the limb of the opposite side, so that she was quite unable to walk without assistance. Exaggeration of the tendon jerks, rectus-clonus and ankle-clonus were all observed. There was also nystagmus, and some tremor in the movements of the upper limbs. Naturally, therefore, the diagnosis was one of disseminate sclerosis. Yet at the present date she is in the enjoyment of fair general health and of a considerable measure of activity. The record of this case thus shows that in individual instances the course of disseminate sclerosis may be a very protracted one. Further, it shows that the more definitely paralytic phenomena of the disease may, to a very large extent, disappear. Remissions and, unfortunately, also, relapses are well-recognized features in the clinical history of disseminate sclerosis. Here, apparently, the lower limbs were largely useless for nearly twelve months at a date so remote as sixteen years ago. Yet no substantial advance of the disease has occurred in the interval, and the patient has for many years been able, though in a restricted and somewhat hesitating fashion, to move about without assistance. The practical lessons from such a record are, first, not to allow such remissions to suggest the diagnosis of functional disease or hysteria; and secondly, to bear in mind the possibility of improvement, even when disseminate sclerosis produces considerable loss of power in one or more limbs. Taken together the five cases show in what varied clinical forms disseminate sclerosis may express itself.

INDEX.

ABDOMINAL pain in gall-stone 197, 198, 361; in pernicious anæmia 329; in pleurisy 361
Abscess of liver (*see* Liver)
Abscess, subdiaphragmatic 335, 336
Accommodation, paralysis of, in diphtheritic paralysis 399; in tabes dorsalis 386
Ague-like attacks (*see* Rigors)
Albuminuria, febrile 363; functional 401; in amyloid disease 333; in diphtheria 399; in cardiac and renal disease 356; in infective endocarditis 307; in renal disease 353, 362, 414
Albuminuric retinitis (*see* Retinitis)
Alcoholic neuritis 369
Amblyopia, crossed 292-300; due to tobacco 429
Amenorrhœa and optic neuritis 85, 90, 127, 138
—— with tumour of brain 90, 95
Amputations, intra-uterine 258-261
Amyloid disease, symptoms of 333
Anæmia and cerebral attacks 84, 114, 117, 128, 138, 418
—— and intra-cranial thrombosis 83, 84, 111-140
—— arterial thrombosis in 133, 144, 311; dropsy in 321; hemianopsia in 128, 130; ocular complications in 111-140; ocular paralysis in 83, 85, 107, 111-140; optic neuritis in 83, 84, 92, 107, 111-140, 418; retinal embolism in 132, 144, 149; retinal hæmorrhages in 136, 324; retinitis in 108; various causes of 353
Anæmia, pernicious 321; abdominal pain in 329; blood in 325; dropsy in 321; enlarged spleen in 326, 327; nervous symptoms in 328; parotitis in 243; pyrexia in 326; retinal hæmorrhage in 136, 324
Anæsthesia, functional 289; in peripheral neuritis and tabes dorsalis 372; in syringomyelia 373
Analgesia, functional 289; in peripheral neuritis and tabes dorsalis 372; in syringomyelia 373
Aneurism and inequality of pupils 234
Ankle-clonus in functional disease 302
Anosmia in functional disease 289; in tumour of brain 96, 98
Argyll Robertson pupil 215-227, 236, 380
Arsenical poisoning 377
Arthritis 1; enlarged bursæ in 15, 16, 34; gonorrhœal 3, 67, 307; in ophthalmia neonatorum 67; in tabes dorsalis 373; myelitica 10; rheumatoid 1-51; sweating 32
Ataxia, in diabetes 235; in diphtheritic paralysis 377; in juvenile
Ataxia in diabetes 235 · in diphtheritic paralysis 377; in juvenile tabes dorsalis 400; in peripheral neuritis 371, 375 (*see* also Tabes dorsalis)
—— pathology of 375

Ataxia, hereditary 397

BABINSKI'S sign (*see* Extensor response)
Beri-beri, œdema in 377
Bladder, loss of power in tabes dorsalis 220, 373; in disseminate sclerosis 427; malignant disease of 157
Blood, in enteric fever 310; in infective endocarditis 314; in pernicious anæmia 325
Blood-pressure and the sphygmograph 163
——— and the sphygmomanometer 163
——— in pulsus alternans 170; measurement of 163
Brain, malignant metastases to 155, 157, 159
Brain tumour, anosmia in 96, 98; knee-jerks in 73, 95-105; ocular paralysis in 157; optic neuritis in 71-110, 115, 157; pseudo-recoveries in 86, 92, 97; recovery from 71-110; sudden death in 81, 86, 92; symptoms of in anæmia 114, 421; treatment in 81, 89; without symptoms 91
Bursæ, enlarged in arthritis 15, 16, 34

CANCER, of breast, metastases from 151; of liver 183, 205, 331; of nervous system 151; of stomach 340, 341
Cardiac murmur, musical 316 (*see* also Endocarditis and Heart disease)
Cerebellar tumour 79, 95 *et seq.* (*see* also Brain, tumour of)
Cerebral embolism (*see* Embolism)
Cerebral symptoms in anæmia 111-140, 421
—— tumour (*see* Brain, tumour of)
Cerebro-spinal fluid in tabes dorsalis 392, 396; in juvenile tabes 400
Chlorosis (*see* Anæmia)
Choroiditis in syphilis 397, 417
Cicatricial constriction of abdomen and thigh 257
Ciliary paralysis in diphtheria 399

DEAFNESS in functional hemiplegia 288
Dexiocardia 345
Diabetes insipidus, ocular paralysis in 237
Diabetes mellitus, Argyll Robertson pupil in 236; ataxia in 235; facial paralysis in 236; hemiplegia in 235; herpes in 236; knee-jerks in 235; neuralgia in 235; ocular paralyses in 237, 239; paralyses in 235; peripheral neuritis in 235; retinal hæmorrhages in 416; retinitis in 235, 416; sciatica in 240
Diphtheritic paralysis 399
Diplopia (*see* Ocular paralyses)
Disseminate sclerosis 425
Doses, official, and drug idiosyncrasies 279
Dropsy (*see* Œdema and Ascites)
Drug idiosyncrasies and official doses 279
Dry mouth 242; and parotitis 242
Dyspnœa 344, 358

EAR, middle-ear disease and ocular paralyses 120, 139; optic neuritis in 120, 139

Embolism, cerebral 156, 310, 313, 315; in infective endocarditis 310-314; in simple endocarditis 314; multiple 313, 314; of limbs 311, 313; of kidney 312, 313; of spleen 309, 312, 313; retinal (*see* Retinal embolism)
Endocarditis, in rheumatoid arthritis 9, 42; prevention of in rheumatic children 61
Endocarditis, gonococcal 319; septic or infective 307; temperature in 314; treatment of 319
Enteric fever, enlarged spleen in 310; leucopenia in 310; ocular paralysis in 139; parotitis in 242; septic endocarditis in 319
Epilepsy, Jacksonian 405
Epileptiform attacks, in brain tumour 76, 104, 105; in functional nervous disease 288
Epistaxis in malignant endocarditis 319
Extensor response, in conditions of unconsciousness 304; in disseminate sclerosis 427; significance of 304, 315

FACIAL paralysis, in cerebellar tumour 96; in diabetes 236; in functional hemiplegia 302
Fever, absence of in abscess of liver 199; in pericarditis 365; in pyo-pneumothorax 343, 351; in pleurisy 362, 365; in pneumonia 365; in septic peritonitis 351
Fever, in cancer of the liver 183; in cirrhosis of the liver 201; in gall-stone 194; in hydatids of the liver 200; in infective endocarditis 307, 317, 318; in pernicious anæmia 53, 326; without physical signs 53, 326
Fever, rheumatic, without arthritis 53
Friedreich's ataxia 397
Functional albuminuria 401
Functional nervous disease (hemiplegia), case of 287, incubation period in 301; motor and sensory phenomena in 287; ankle-clonus in 302; visual fields in 291
Fundus oculi, opaque nerve fibres in 251 (*see* also references under Ophthalmoscopic examination)

GAIT, in disseminate sclerosis 426; in functional hemiplegia 305; in peripheral neuritis 371; in tabes dorsalis 372
Galenical preparations 277
Gall-bladder, enlargement of in malignant disease of the liver 190
Gall-stone, absence of pain in 197; diagnosis from malignant disease of liver 190-199; impacted 194; rigors in 194; temperature in 194
Gall-stone pain, simulated by pernicious anæmia 329; by pleurisy 361
Gangrene of lung as cause of pneumothorax 343
Gastric ulcer, and parotitis 245, 246; malignant 340, 341
Gonococcal endocarditis 319
Gonorrhœal rheumatism 3, 67, 307; subcutaneous nodules in 51
Gout, and rheumatoid arthritis 35, 37; Heberden's nodes in 41; subcutaneous nodules in 35, 37, 50
Greyness of hair, sudden 623
Gumma, of brain 405; of liver 404; periosteal 408; subcutaneous 403; with tabes dorsalis 410

HÆMATEMESIS, and optic neuritis 119; and parotitis 245, 246; in cirrhosis of the liver 201; in infective endocarditis 319; in pernicious anæmia 329
Hæmaturia, as only symptom of malignant disease of bladder 157; in renal embolism 312, 319
Hæmorrhages and optic neuritis 119
Hair, sudden greyness of 263
Haygarth on nodosity of the joints 5, 14
Headache, in intra-cranial tumour 73, 100, 158, 406; in renal disease 414, 422
Heart disease, in rheumatoid arthritis 9, 42; prevention of in rheumatic children 61; retinal embolism in 148; septic or infective 307
Heart, displacement of 345
Heberden's nodes 14, 41
Heel-jerk, absence of in tabes dorsalis 381, 410
Hemianæsthesia, functional 289
Hemianalgesia, functional 289
Hemianopsia, in anæmia 128, 130; in brain disease 159, 295; with hemiplegia 295 (*see* also Visual field)
Hemiplegia, in anæmia 84, 115, 138; in diabetes 235; with hemianopsia 295
Hemiplegia, functional 287; crossed amblyopia in 292; facial paralysis in 302; gait in 305; hemianæsthesia in 289; hemianalgesia in 289; incubation period in 301; in disseminate sclerosis 430; knee-jerks in 303; plantar reflexes in 304; treatment of 304
Herpes in diabetes 236
Hippocratic succussion 344
Hypermetropia and optic neuritis 127, 138
Hysterical paralysis (*see* Hemiplegia, functional)

IDIOSYNCRASIES to drugs 279
Influenza, and parotitis 245; and subcutaneous nodules 51
Internal capsule, lesions of 290
Intra-cranial syphilis 387, 405
Intra-uterine amputations 258, 261
Irides, difference in colour of 229
Iridoplegia 222, 229
Iritis, in acquired syphilis 408; in inherited syphilis 391

JACKSONIAN epilepsy 405
Jaundice in cancer of the liver 185-192, 205, 232
—— in gall-stone 194-199; in hypertrophic cirrhosis 202
Joints, diseases of (*see* Arthritis)
—— embolism of 312; in infective endocarditis 316; in tabes dorsalis 373
Juvenile tabes dorsalis 392, 398

KERATITIS, interstitial 99, 391, 395, 408, 418
Kidney (*see* Nephritis)
Knee-jerks, in diabetes 235, 240; in diphtheritic paralysis 399; in disseminate sclerosis 427; in Friedreich's ataxia 397; in functional nervous disease 303; in intra-cranial throm-

bosis 116; in intra-cranial tumour 73, 95-106; in juvenile tabes 400; in meningitis 117; in peripheral neuritis 371, 375; in rheumatoid arthritis 46; in syringomyelia 373; in tabes dorsalis 215-227; methods of testing 73, 372; persistence of in malignant disease of spinal cord 155; reinforcement of 372
—— absent without other signs of disease 398

LEUCOCYTOSIS in infective endocarditis 314
Leucopenia, in enteric fever 310; in pernicious anæmia 325
Liver, abscess of 199, 335, 336, 347
—— cancer of 185, 205, 331; enlarged spleen in 205, 331; rigors in 190; temperature in 183, 335
—— cirrhosis of 201; enlarged spleen in 201, 333; hæmatemesis in 201; temperature in 201
—— displacement of 334, 345
—— enlarged left lobe of 209, 210, 213, 404
—— enlargement of, causes of 206, 207
—— gumma of 404; syphilis of 333, 404
Locomotor ataxia (*see* Tabes dorsalis)
Lumbar puncture (*see* Cerebro-spinal fluid)
Lung, disease of, and pneumothorax, 347
—— gangrene of 343
—— malignant disease of 154, 159

MALARIA, retinal hæmorrhages in 324
Meningitis or cerebral tumour, differential diagnosis 76
Metastases in malignant disease 151; to nervous system 151
Middle-ear disease, and ocular paralyses 120, 139; and optic neuritis 120, 139; fundus oculi in 139
Mouth, dry 242; and parotitis 242
Mumps (*see* Parotitis)
Muscular atrophies, in rheumatoid arthritis 46; in syringomyelia 373
—— hyperæsthesia, in peripheral neuritis 375
—— sense 289, 375

NEPHRITIS, chronic interstitial 354, 361, 414; in young adults 422; pericarditis in 365; pneumonia in 365; pleurisy in 365; retinitis in 238, 357, 358, 414, 422; treatment of 359, 366, 415
Nerve-deafness, functional 288
Neuralgia, in diabetes 235; in tabes dorsalis 379; with sudden greyness of hair 264
Neurasthenia, traumatic 287
Neuritis (*see* Peripheral neuritis)
Neuritis, retrobulbar (*see* Retrobulbar neuritis)
Nodes, Heberden's 14, 41
Nodosity of the joints, 5, 14
Nodules, subcutaneous 1-51; in gonorrhœal arthritis 51; in gout 35, 37; in rheumatism 1, 12, 18, 62; structure of 25, 31; without joint disease 51
Nystagmus, in disseminate sclerosis 427, 428; in Friedreich's ataxia 397; in intra-cranial tumour 97, 105

OCULAR complications in anæmia 83, 107, 111-140, 418
Ocular paralyses in anæmia 83, 107, 111-140; in diabetes 237, 239; in diphtheritic paralysis 399; in disseminate sclerosis 427; in enteric fever 139; in intra-cranial thrombosis 84, 111-140, 157: in juvenile tabes dorsalis 398; in middle-ear disease 120, 139; in tabes dorsalis 215, 386. 398; in tumour of brain 158; treatment of 389
——— due to cold 217, 233, 386; due to rheumatism 217, 233, 386
Œdema, causes of 321, 370, 377; in kidney disease 355; in pernicious anæmia 321; in peripheral neuritis 236, 324, 369; in tabes dorsalis 373
Official doses and prescribing, 279
Opaque nerve fibres in retina, 251
Ophthalmia neonatorum and arthritis 61
Ophthalmoscopic examination in medical cases 413 (*see* also Anæmia, cerebral tumour, diabetes, middle-ear disease, opaque nerve fibres, optic atrophy, optic neuritis, pernicious anæmia, retinal embolism, retinal hæmorrhages, retinitis)
Optic atrophy, in anæmia 420; in brain tumour 74, 105, 110; in disseminate sclerosis 428; in tabes dorsalis 215, 383, 393; treatment of 390; without other evidence of disease 219
—— post-neuritic and primary 75, 106
Optic neuritis, after hæmorrhages 119; in anæmia 83, 84, 92, 107, 111-140, 418; in children and young adults 71; in hypermetropia 127, 138; in intra-cranial thrombosis 83, 84, 111-140, 156; in intra-cranial tumour 71-110, 115, 157, 419; in middle-ear disease 120, 139; retrobulbar 87, 109, 133; second attacks of 103, 104; with amenorrhœa 85, 90, 127, 138
Optic neuritis and loss of sight 87, 109, 135, 419
Osteo-arthritis (*see* Rheumatoid arthritis)
Osteophytes 14

PAIN, abdominal, in gall-stone 197, 198, 361; in pernicious anæmia 328;; in pleurisy 361
Pain in malignant disease of spinal cord 151, 153, 154; in peripheral neuritis 369, 374, 375; in syphilis 408; in tabes dorsalis 380, 382
Papillitis (*see* Optic neuritis)
Paralyses in disseminate sclerosis 430, 431, 432
Paralysis, diphtheritic 399
—— facial, in cerebellar tumour 96; in diabetes 236; in functional hemiplegia 302
—— functional (*see* Hemiplegia)
—— ocular (see Ocular paralysis)
—— of palate in diphtheria 399
Parotid, swelling of (*see* Parotitis)
Parotitis 241; and menstruation 241; after abdominal operations 242, 243; complicating various diseases 241, 242, 244; in gastric ulcer 245, 246; in hæmatemesis 245, 246; in influenza 245; in pneumonia 246, 247; in purulent peritonitis 247
Perforating ulcer of sole, in diabetes 236; in tabes dorsalis 236, 410

Pericarditis in renal disease 365
Perimeter charts (*see* Visual fields)
Periostitis in syphilis 406
Peripheral neuritis 325, 369; ataxia in 371, 375; gait in 371; causes of 376; in diabetes 235; in diphtheria 399; in pernicious anæmia 328; œdema in 321, 370, 376; sensory defects in 372; treatment of 378
Pernicious anæmia (*see* Anæmia, pernicious)
Pharmacopœia, the authority and purpose of 269
—— official doses in 279
Phthisis pulmonalis, and pneumothorax 347; and tumour of brain 100
Plantar reflex, in cerebral embolism 309, 312; in functional paralysis 304; in unconscious states 304; significance of extensor response 304, 315
Pleural effusion, in cancer of liver 334; in impacted gall-stone 194; in infective endocarditis 314; in malignant disease of lung 154; in pneumothorax 349; in pyo-pneumothorax 344
Pleurisy, in malignant disease of lung 154; in renal disease 361
Pneumonia, in renal disease 365; parotitis in 246, 247
Pneumothorax, causes of 346; treatment of 350
Position, loss of the sense of 289
Ptosis, in diabetes 237; in tabes dorsalis 223, 386
Pulse, loss of in embolism 309, 311
Pulse-tracings and blood-pressure 163
Pulsus alternans 170
Pupils, Argyll Robertson 215-227, 236; in aneurism 234; in chest diseases 234; inequality of 229-234, 380, 392; in organic nervous disease 233; in tabes dorsalis 215, 381, 392; in juvenile tabes 400; irregularity from iritis 408; paralysis of 222, 227, 229, 399
Pyo-pneumothorax, case of 343; causes of 346
Pyrexia (*see* Fever)

REINFORCEMENT of tendon jerks 372
Respiration, frequent 343, 358
Retina, opaque nerve fibres in 251
Retinal embolism, in anæmia 132, 144, 149; in mitral disease 148; visual fields in 145, 146, 149; with normal central vision 141
Retinal hæmorrhages, in anæmia 136, 323; in diabetes 416; in malaria 324; in various diseases 323, 324
Retinitis, albuminuric 238, 357, 358, 414; in anæmia 108, 422; in diabetes 235, 238, 416
Retrobulbar neuritis 87, 109, 133
Rheumatic children, prevention of heart disease in 61
Rheumatic fever without arthritis 53
Rheumatic gout (*see* Rheumatoid arthritis)
Rheumatic nodules 1, 12, 18, 38; varieties of 20, 39
Rheumatism, anæmia in 355; gonorrhœal 3, 67, 307; ocular paralysis from 217, 233; origin of name 1; in early life 12, 62, 67
Rheumatism and rheumatoid arthritis 1-51

Rheumatoid arthritis 1-51; cardiac disease in 9, 42; enlarged bursæ in 15, 16, 34; exaggerated tendon phenomena in 46; muscular atrophies in 46; subcutaneous nodules in 1, 18, 22, 25, 28, 30, 33, 34, 35, 38; various symptoms in 32
Rigors in diseases of the liver 183; in gall-stone 194; in infective endocarditis 319
Romberg's test 371, 429

SCIATICA, in diabetes 240; in tabes dorsalis 222, 382
Sensation, defective, in functional nervous disease 288; in peripheral neuritis 372; in syringomyelia 372; in tabes dorsalis 372, 373, 374, 385
Sight in optic neuritis 87, 109, 135, 419; in optic atrophy 382, 393
Speech in disseminate sclerosis 427
Sphygmograph and blood-pressure 163
Sphygmomanometer and blood-pressure 163
Splashing sound 344
Spleen, cancer of 208
Spleen, enlargement of, causes of 207, 208; in amyloid disease 333; in cancer of liver 205, 331, 340; in cirrhosis of liver 201, 334; in enteric fever 310; in hydatids of liver 337; in impacted gall-stone 195, 207; in infective endocarditis 309, 314; in pernicious anæmia 326, 327; simulated by enlarged left lobe of liver 209; by malignant tumour of stomach 210, 212
Stomach, cancer of 340, 341; simulating enlarged spleen 211
Subcutaneous nodules (*see* Nodules, subcutaneous)
Sub-diaphragmatic abscess 335, 336
Succussion, Hippocratic 344
Sweating arthritis 32
Syphilis, aortic disease in 409; gummata in 403; intra-cranial 387, 405; in married women 403; iritis in 408; of liver 333, 404; periostitis in 407; Wassermann reaction in 396
—— and ocular paralyses 222, 387
—— and tabes dorsalis 387, 395, 398, 403
Syphilis, inherited, choroiditis in 397, 417; iritis in 391; juvenile tabes dorsalis in 395, 398; keratitis in 99, 391, 395, 408, 418; periostitis in 407; teeth in 395, 417
Syringomyelia, symptoms of 373

TABES DORSALIS, cases of 215, 379, 391, 410; cerebro-spinal fluid in 392; eye symptoms in 215, 373, 386; gastric crises in 410; in women 391; joint affections in 373; juvenile form 392, 398; neuralgia, tabetic 379; ocular paralyses in 215, 386, 398; œdema in 373; optic atrophy in 215, 383, 393; pains in 379, 398; pupils in 215, 381, 392; periostitis in 410; sensory defects in 372, 374, 385; sex distribution of 391; symptoms of 215, 373, 374, 379, 380, 392; treatment of 388; without ataxia 379
Tabes dorsalis and peripheral neuritis, differential diagnosis 372
Tabetic neuralgia 379
Teeth in inherited syphilis 395, 417

Temperature (*see* Fever)
Thrombosis, arterial 133, 144, 311
—— sinus and venous, in anæmia 84, 111-140, 144; optic neuritis in 83, 84, 111-140, 156, 421
Tobacco amblyopia 429
Traumatic neurasthenia 287
Tremor in disseminate sclerosis 426
Tricuspid regurgitation 354, 356

UMBILICAL CORD, constriction by 257
Uterine hæmorrhages and optic neuritis 119

VISCERA, metastases following malignant disease of 151
Vision (*see* Sight)
Visual fields (*see* also Hemianopsia) in crossed amblyopia 292; in diabetes 237, 239; in functional hemiplegia 291, 296; in intra-cranial thrombosis 128, 130; in retrobulbar neuritis 134; in retinal embolism 144, 146, 149; in tabes dorsalis 383, 393
—— for colour in functional hemiplegia 294
Visual path, diagram of 293
——— lesions of 291, 297
Vomiting in intra-cranial tumour 72, 74, 95, 100

XEROSTOMIA 242, 248

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